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CONTENTS

The Influence of Past Experience in Visual Perception: K. W. BRALY..... 613

A Study of Tremor in Normal Subjects: I. C. YOUNG 644

The Relation between Length and Difficulty in Motor Learning: A Comparison with Verbal Learning: T. C. SCOTT AND L. L. HENNINGER..... 657

Studies in Cross Education: II. Further Experiments in Mirror Tracing the Star-Shaped Maze: T. W. COOK..... 679

Some Records of the Memorizing of Sonnets: K. GORDON..... 701

An Experimental Study of Composer-Preferences of Four Outstanding Symphony Orchestras: E. E. E. FOLGMANN..... 709

A Study of the Development of Tolerance for Caffeinated Beverages: A. L. WINSOR AND E. I. STRONGIN 725

Changes in Grip Tension following Electric Shock in Mirror Tracing: W. MCTEER 735

The D. L. for Cutaneous Two-Point Stimulation by the Method of Single Stimuli: F. D. FRY, D. D. M. HAUPT AND L. WARTENA..... 743

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THE INFLUENCE OF PAST EXPERIENCE IN VISUAL PERCEPTION *

BY KENNETH W. BRALY

PROBLEM

The object of this investigation was a study of the rôle of past experience in perception, or more specifically, a determination of the influence of the perception of certain visual forms upon the subsequent perception of other visual forms.

The procedure consisted of repeated tachistoscopic exposures of geometrical forms of a given type as an 'impression-series' followed by a 'test-series' consisting of additional geometrical forms of different types interspersed with the initial ones. For example, in one experiment (Experiment III), various arrangements of squares were given as an impression-series followed by test-slides containing triangles or pentagons. In this way it was hoped to discover whether there would be a perseveration of the type of figure experienced in the impression-series in the perception of the forms in the test-series.

HISTORY

Past experience has been an important explanatory principle in most systems of psychology. Especially has this been true of the treatments of visual perception where the manner in which a stimulus object is perceived has been regarded as in a large degree a function of the preceding experiences. The

* From the Psychological Laboratory of Princeton University.

validity of this view, however, has been severely questioned by writers of the Gestalt school.[1] Wertheimer,[2] for example, concealed familiar forms, such as letters of the alphabet, in complex configurations, and found that it was often difficult to see the letters even when the observer knew of their presence. He concluded that past experience was ineffective, since the letters presumably were more favored by past experience than were the complex and relatively strange configurations in which they were concealed. Likewise Köhler says, "The more one avoids the rather common but dangerous phrase, that this or that thing 'must of course be explained by experience,' the more one tries, in our case, for instance, to consider concretely how experience might produce units (and out of what?), the less plausible becomes this rather superficial statement. . . . We are surprised to see how easily even that effect of experience which I have admitted is overcome by showing a well-known form in a constellation in which the primary tendencies are working against our seeing that form." [3] Elsewhere the same writer says, "I find a decisive argument against explanations of form in terms of past experience, in the following consideration. No doubt parts of the visual field can reproduce past experiences associated with them in previous life. But when we ask what factors are the main reproductive properties in these cases, we see that in 99 out of 100 cases *reproduction depends upon the form of a segregated whole in the field.*" [4]

The Gestalt position has been substantiated by an experimental study by K. Gottschaldt, in which he sought to determine the extent to which the perception of form is a function of past experience.[5] His method consisted of repeated ex-

[1] A recent formulation of the traditional view and the one generally referred to by members of the Gestalt school is that of G. E. Müller, *Komplextheorie und Gestalttheorie*, 1923.

[2] Max Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1923, **4**, 332–337.

[3] Wolfgang Köhler, An aspect of Gestalt Psychology, *Ped. Sem.*, 1925, **32**, 710–711.

[4] *Gestalt Psychology*, 1929, 212.

[5] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. I. Über den Einfluss gehäufter Einprägung von Figuren auf ihre Sichbarkeit in umfassenden Konfigurationen, *Psychol. Forsch.*, 1926, **8**, 261–317.

posures of certain figures as an impression-series, and then the presentation of test-figures in which the impression-figures were concealed. The extent to which the impression-figures emerged from their concealment and were recognized in the new complexes was taken as the measure of the influence of past experience. In the main series Gottschaldt employed 5 outline figures as impression-forms, and as test-figures 31 complicated geometrical patterns. The impression-figure was concealed by various means such as by employing only a part of its contour as the contour of the dominant form within the new complex, or by covering it with several lines which formed another pattern. To one group of subjects each of the impression-figures was given 3 times in exposures of one second duration, and then the 31 test-figures were given in a mixed order, each for two seconds. To another group each of the impression-figures was given 520 times, and then the test-figures were presented. The subjects were instructed to study and memorize the impression-figures, and were encouraged to designate the figures by descriptive names. They were required during the test-series to give a detailed verbal description of the figures. The results showed that an impression-figure was recognized within its test-pattern in only 6.6 percent of the cases for the group given 3 exposures of the impression-figures, and in but 2.9 percent of the cases for the group given 520 exposures. Intermediate numbers of exposures likewise failed to give increased frequency of recognition. Comments of the observers led Gottschaldt to conclude that such recognition as occurred was the result of an attitude of expectancy or a tendency to 'search.' This conclusion was strengthened by results from later experiments in which the observers were instructed to search for the impression-figures. The frequency of recognition was increased, but was still limited by the degree of 'coherence' of the total complex. In a later paper [6] the 'searching attitudes' were shown to vary greatly with slight changes in the experimental conditions.

[6] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. II. Vergleichende Untersuchungen über die Wirkung figuraler Einprägung und den Einfluss spezifischer Geschehensverlaufe auf die Auffassung optischer Komplexe, *Psychol. Forsch.*, 1929, 12, 1–87.

If two different impression-figures were presented alternately, then a complex test pattern might be perceived as containing one of the impression-figures if the same order were maintained for the test-patterns as prevailed for the impression-figures, but not if the order were changed. Similar results were obtained with even more complex modes of presentation if the stimuli of the test-series followed strictly the rhythm of expectancy established during the impression-series.

From his results Gottschaldt concludes that in the perception of form the essential factors are 'situation-vectors' or attitudes of expectancy within the individual resulting from the instructions or experimental procedure, and the inherent properties of the stimulus which may serve to limit the influence of such situation-vectors. The influence of experience itself is denied, for although the situation-vectors may be 'after-effects' of the preceding training or procedure, they are not specific to certain forms, and do not vary with the amount of repetitive training with that particular figure.

Gottschaldt's experiments were received with enthusiasm by other workers of the school. Köhler's comment was, "After these results, whoever defends the automatic influence of experience upon our seeing definite forms will have incumbent upon him the task of supporting his theory by other experiments." [7]

It is plain that Gottschaldt's method was hardly a complete test of the rôle of past experience in perception. As Moore has pointed out,[8] the method of submerging the impression-figure in the test-pattern demonstrates little more than the methods by which similarity to the isolated figure may be destroyed. His results, in other words, are applicable less to the problem of the rôle of past experience in perception than to the essentially separate problem of visual units as determined by lines, contours, and spatial relations. His work shows, to quote Moore, that "naive *Os* do not go beyond the contours as seen; trained *Os* may attempt to do so with but indifferent success."

[7] *Op. cit.*, 208. See also Koffka's discussion, Kritik der Erfahrungshypothese, Psychologie der Optischen Wahrnehmung, *Hdbh. d. Norm. u. Pathol. Physiol.*, 1930, **8**, 1224–1227.

[8] M. G. Moore, Gestalt *vs.* experience, *Amer. J. Psychol.*, 1930, **42**, 453–455.

In the present investigation an attempt was made to avoid the limitations of Gottschaldt's method (1) by confining the stimuli to very simple figures, and (2) by keeping the impression-figures distinct from the test-figures instead of concealing them in complex patterns.

Experiments

(1) *Stimulus Material.*—The exposure material, presented on tachistoscopic slides, consisted of simple geometrical forms. These forms were composed of arrangements of dots representing triangles, quadrilaterals, and pentagons. The tachistoscopic slides

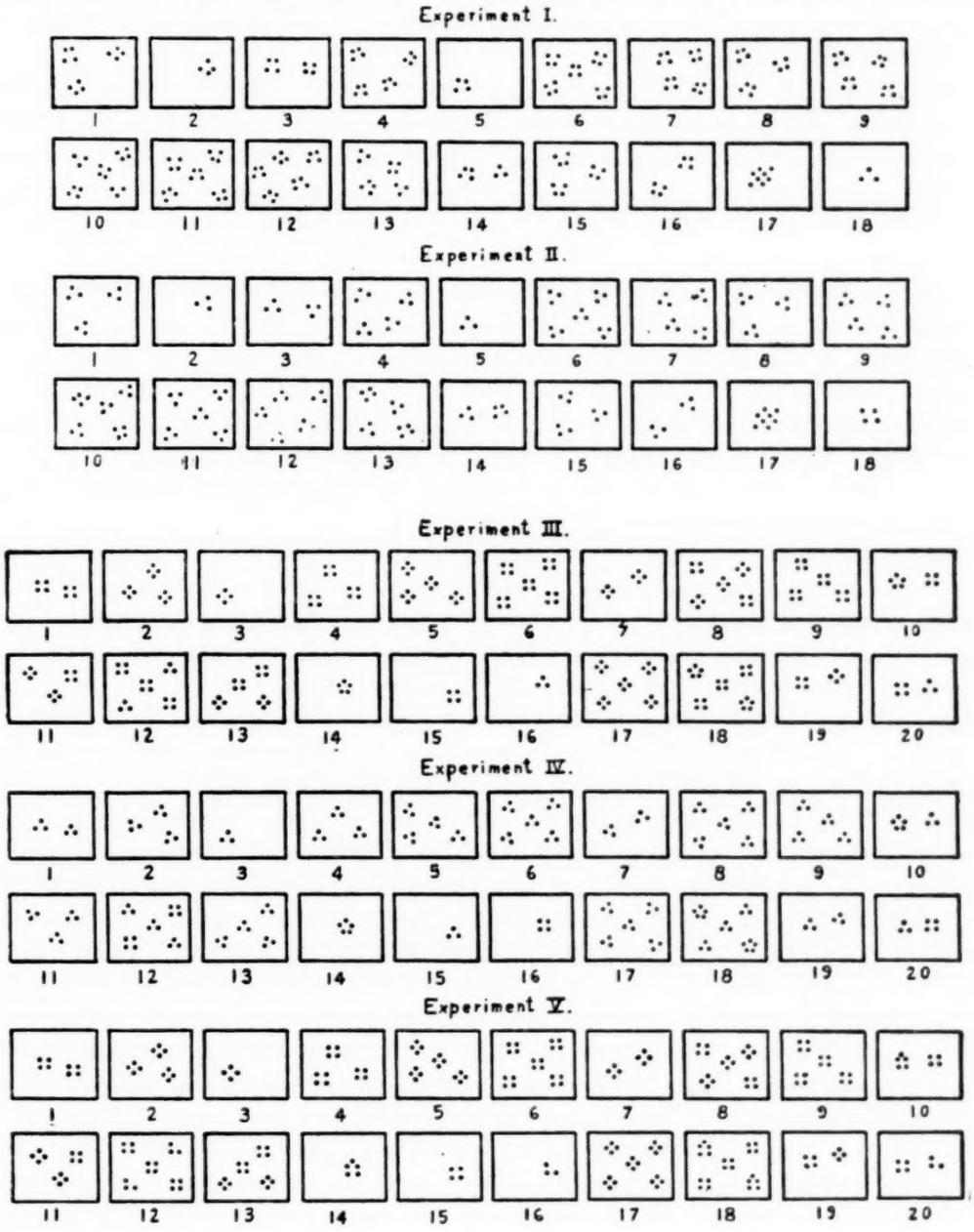

Fig. 1.

used in the experiments on past experience (Experiments I to V) are shown in Fig. 1. The slides are represented in horizontal rows in order of presentation. The first nine slides for each experiment comprised the impression-series, and the last nine (or eleven) slides comprised the test-series. Each impression-series contained various numbers and arrangements of a single geometrical form. The test-series which immediately followed consisted of three types of slides: (1) slides which contained only the impression-figure; (2) slides which contained only new figures not shown in the impression-series; and (3) slides which contained both old and new figures. For example, in Experiment I, the first nine slides (impression-series) contained triangles only; while the remaining slides (test-series) contained triangles, quadrilaterals, or both triangles and quadrilaterals. The number and arrangement of figures varied from slide to slide. The slides containing only the new figure and the slides containing the combination of new and impression-figures were used to determine the influence of past experience. These are called the test-slides.

(2) *Observers.*—In each of the first five experiments 20 observers were given the impression- and test-series and 20 observers were given only the test-slides. The observers who were given only the test-slides served as controls to check the perception of the test-slides without the immediately preceding experience of the impression-figures. Due to the nature of the problem no observers were used on more than one of these first 5 experiments.

(3) *Apparatus.*—The tachistoscopic exposures of the stimulus material were made by means of a projection lantern. The slides were illuminated by a high-speed lamp. The duration of the exposure was approximately 0.175 second. Although the lamp had a power of 250 watts, the intensity of the illumination was diminished by the distance from the screen (9 feet), and the interposition of opal glass between the lamp and slide. The experimental room was also dimly illuminated.

(4) *Procedure.*—In all of these experiments the observers were first requested to read the following instructions:

"You will be shown a series of slides which you are to draw on a proportionately reduced scale on slips of paper, one slip being used for each slide. The slides will show a group or groups of dots, so arranged, however, that they may be taken as representing the 'corners' of simple geometrical figures. In place of drawing the dots, sketch the figure or figures in their relative positions. In case the dots cannot readily be seen as indicating a particular figure, but only then, are you to draw the dots in their proper position. Each time you have finished drawing what you have seen and are ready for the next exposure, signal (by saying 'ready') and a new slide will be shown. (As a preliminary preparation you will be shown a 'blank' exposure several times and you are to attempt to see this as a 'whole' each time. This should accustom you to the position and size of the screen picture, and to the length of exposure-time employed.)"

The observers were seated at the table in front of the screen, shown some blank exposures, and then three slides giving complicated outline figures. This was done merely to allow for a certain degree of adjustment to the experimental conditions, as well as to permit visual adaptation to the low illumination of the room. The observers were given blank slips of paper (the size of the unprojected slides) upon which to draw their reproductions. The procedure as outlined in the instructions given above was then carried out.

All observers were relatively unfamiliar with tachistoscopic work. All served without knowledge of the purpose of the investigation. Volunteered comments gave repeated evidence that for them it was merely a test to determine their ability to copy briefly exposed material.

(5) *Discussion of Method.*—Simple geometrical forms were selected (1) so that the reproduction of the impression-series would be as correct as possible and thus establish a uniform factor of immediate past experience in the different observers, and (2) so that the reproductions would show only a few unambiguous variations which could be classified and compared. Therefore, the experiments were confined to triangles, quadrilaterals, and pentagons.

The figures were represented by arrangements of dots, because the perception of groups of dots for any given figure was assumed to depend more upon subjective, attitudinal factors, influenced by past experience, than was the perception of outlined figures. This assumption itself was put to test in the investigation, and checked through the use of line-figures in later supplementary experiments.

In the procedure it was considered desirable to avoid so far as possible any change in the mental attitude of the observer toward the impression- and the test-series, and at the same time to have these series vary in difficulty so that the impression-series would be nearly perfectly reproduced and the test-series imperfectly reproduced.[9] In an effort to attain this end, the test-series was kept the same as the impression-series except for the substitution of new figures.[10]

Results

The graphic reports may be considered as regards both the extent of variations from perfect reproduction and the types of these variations. In the first five experiments, the reports for the impression-series indicate the extent to which presentation of those series had established a factor of past experience of a given type. The reports of the same observers for the test-series, taken in comparison with the results from the

[9] Only by the latter means can one study the influence of factors other than the immediate stimulus. For a discussion of this point see James J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 1929, **12**, 5–6.

[10] The substitution of the new figures was relied upon to increase the difficulty sufficiently, by decreasing the span of apprehension. Although the work upon the span of apprehension is often treated as if the span depended solely upon the number of units, nevertheless, it is generally recognized that the use of heterogeneous units decreases the span.

control groups, indicate effects attributable to past experience provided by the impression-series.

It is necessary, before considering the main results, to determine the accuracy of reproduction of the impression-series in order to evaluate the effectiveness of that series in establishing a factor of past experience.

Accuracy of Reproduction of Impression Series.—The impression-series for all five experiments consisted of presentations to each observer of 9 impression-slides. The reproductions of these slides, for the 20 observers taken as a group, are analyzed in Table I. The first column shows the types of variation from perfect reproduction which were (1) substitu-

TABLE I

VARIATIONS IN REPRODUCTIONS OF THE IMPRESSION-SERIES

1 Type of Variation	2 No. Reports Showing Variations	3 Percent of Reports with Variations (Percent of 180)	4 Total No. of Figures Presented	5 No. of Variation-Figures	6 Percent of Total No. Presented
Experiment I					
Substitution of triangles and pentagons	25	13.9%		53	9.8%
Omission of quadrilaterals	15	7.3%	540	43	8.0%
Addition of quadrilaterals	7	3.9%		8	1.5%
				104	19.3%
Experiment II					
Substitution of quadrilaterals	3	1.6%		4	0.7%
Omission of triangles	14	1.7%	540	20	3.7%
Addition of triangles	3	1.6%		4	0.7%
				28	5.1%
Experiment III					
Substitution of triangles	4	2.2%		4	0.7%
Omission of squares	6	3.3%	580	6	1.0%
Addition of squares	4	2.2%		5	0.9%
				15	2.6%
Experiment IV					
Substitution of quadrilaterals	12	6.6%		18	3.1%
Omission of triangles	14	7.7%	580	24	4.2%
Addition of triangles	0	0.0%		0	0.0%
				42	7.3%
Experiment V					
Substitution of triangles	5	2.8%		5	0.9%
Omission of squares	8	4.4%	580	11	1.9%
Addition of squares	7	3.9%		7	1.2%
				23	4.0%

tions of figures other than the stimulus-figure, (2) omissions from the correct number of stimulus-figures, and (3) additions beyond the correct number of stimulus-figures.[11] Column 2 shows for each experiment the number of reports in which one or more variations occurred. Reports showing variations of more than one type are listed more than once, and hence the listed number of reports showing variations is greater than the actual number. Column 3 gives these values as percentages of the total number of presentations, 180 for each experiment. Columns 4, 5, and 6 deal with the individual figures themselves; column 4 shows the total number of figures presented, column 5 the number of figures for each type of variation, and column 6 the number of these latter 'variation-figures' expressed as percentages of the number of figures presented.

As shown in the last column of the table, the percentages of variation-figures in the five experiments were as follows: Experiment I, 19.3 percent; Experiment II, 5.1 percent; Experiment III, 2.6 percent; Experiment IV, 7.3 percent; and Experiment V, 4.0 percent. It should be noted that in Experiment I the substitutions of new figures, *i.e.*, qualitative variations, were slightly more frequent than omissions or additions of figures, *i.e.*, quantitative variations, whereas in the remaining experiments the contrary was the case. This difference between Experiment I and the remaining experiments appears somewhat more clearly in the percent of reports showing variations (column 3).

A comparison of the number of variations with the number of reports in which these occurred shows that the variations were not concentrated in a few slides, but were fairly well scattered. Hence the past experience established by these impression-series may be taken as a reliable function of the figures presented, and may therefore be used in relation to the test-series to be described.

Types of Reproduction in the Test Slides.—As mentioned above, to one group of observers, called the experimental

[11] Displacements from correct position were not considered. Four cases in Experiment I in which pentagons were added to the total number of quadrilaterals are listed as substitutions of pentagons.

group, the test-series was presented immediately following the impression-series. To another, the control group, only the test-series was given. A comparison of the results of these two groups, therefore, will show the effects of the impression-series. Table II gives the total number of individual

TABLE II

NUMBER OF FIGURES REPORTED FOR THE TEST SLIDES
(The figures employed in the impression-series are italicized)

1 Type of Figure	2 Total No. Presented	3 Total No. Experimental Group	4 Reported Control Group	5 Ratio of Reported to Presented Figures: Experimental Group	6 Ratio of Reported to Presented Figures: Control Group	7 Difference (in Percent) in Reports by the Two Groups
Experiment I						
Quadrilaterals	120	161	68	134%	56%	87%
Triangles	120	56	117			
Others	0	15	48			
Experiment II						
Triangles	120	165	112	138%	93%	45%
Quadrilaterals	120	59	69			
Others	0	13	24			
Experiment III						
Squares	160	204	159	128%	99%	29%
Triangles	80	58	52			
Pentagons	80	51	59			
Others	0	3	21			
Experiment IV						
Triangles	160	169	104	106%	65%	41%
Squares	80	54	67			
Pentagons	80	43	62			
Others	0	34	46			
Experiment V						
Squares	160	207	219	129%	137%	−8%
Triangles	80	55	44			
Pentagons	80	51	49			
Others	0	1	6			

figures reported for the test-slides by experimental and control observers. The results are given separately for the five experiments. The types of figures reported are shown in column 1, with the type of figure which had been employed in the preceding impression-series indicated by italics. The

'others' designation includes types of figures not presented in the test-series. In column 2 is given the number of figures of each type presented to each group of observers. Columns 3 and 4 state the number of figures of different types reported by the experimental and the control groups respectively. In columns 5 and 6 are given, for experimental and control groups respectively, the number of reports, in percent, of figures of the type that had been used in the impression-series.

It will be noted that the observers of the experimental group reported more figures of the impression-type than had been presented to them, while the observers of the control group, in four out of the five experiments, reported fewer figures of this type than had been presented. These differences are shown in column 7. It seems safe to say that the increased number of impression-figures given by the experimental groups of Experiments I to IV is due to the influence of the impression-series. Experiment V failed to show such an effect because of the particular nature of the figures used, as will be discussed later.

Since additions of figures occurred but rarely (in but three instances in the five experiments), the above results are chiefly due to omissions of presented figures or substitutions of different figures for those presented. The substitutions are considered in detail in Table III.

Column 1 of Table III shows separately the slides which contained test-figures. Columns 2 and 3 show the number of substitutions reported by experimental and control groups, respectively. Columns 4 and 5 show for the two groups the percentages of test-figures for which substitutions were reported. Column 6 shows the differences between experimental and control groups (*i.e.*, the differences between the percentages given in columns 4 and 5).

Substitutions of impression- for test-figures were made by both groups of observers, but to a significantly greater extent by the experimental group in all but Experiment V. An inspection of the construction of the slides as shown in Figs. 1 and 2 reveals that several of the test-slides contained only one or two figures (*i.e.*, Nos. 14, 17, and 18 for Experiments

TABLE III

NUMBER OF SUBSTITUTIONS OF IMPRESSION-FIGURES FOR TEST-FIGURES IN THE TEST-SLIDES

1 Stimulus Slide	2	3	4	5	6 Difference between the Two Groups
	No. of Substitutions of the Impression-Figure		Percentage of Test-Figures Substituted for		
	Experimental Group	Control Group	Experimental Group	Control Group	
Experiment I					
No. 10	29	6			
No. 13	28	13			
No. 14	6	7			
No. 17	12	13			
No. 18	—	—			
Total	75	39	47%	24%	23%
Experiment II					
No. 10	32	19			
No. 13	28	18			
No. 14	3	5			
No. 17	16	11			
No. 18	—	—			
Total	79	53	49%	33%	16%
Experiment III					
No. 10	2	3			
No. 12	22	21			
No. 14	—	1			
No. 16	—	—			
No. 18	23	11			
No. 20	1	—			
Total	48	36	30%	23%	7%
Experiment IV					
No. 10	—	2			
No. 12	32	21			
No. 14	—	—			
No. 16	—	—			
No. 18	10	1			
No. 20	—	—			
Total	42	36	26%	15%	11%
Experiment V					
No. 10	2	5			
No. 12	28	37			
No. 14	—	—			
No. 16	—	—			
No. 18	27	28			
No. 20	—	1			
Total	57	71	36%	45%	−9%

I and II, and Nos. 10, 14, 16, and 20 for Experiments III, IV, and V). These slides were so simple that in the reports upon them practically no substitutions were made. If these were to be disregarded, the differences between the results for the experimental and the control groups would be even more pronounced. Nevertheless, the above data demonstrate clearly the influence of the impression-series on the reports for the test-series in the first four experiments.

The influence of the impression-series has been shown both in the number of figures of the impression-type reported in the test-series and the number of such figures substituted for the test-figures. A comparison of Tables II and III will disclose further that the increased number of impression-figures reported by the experimental groups was due both to a greater number of substitutions and to a greater number of reports of such figures when they were presented. Thus in Experiment I the experimental group substituted the impression-figure for 37 more test-figures than did the control group, but gave 93 more figures of the impression-type. Similarly in Experiment II the experimental group gave 21 more substitutions of the impression-figure than did the control group, but 51 more figures of the impression-type; in Experiment III, 12 more substitutions and 45 more impression-figures; in Experiment IV, 18 more substitutions and 65 more impression-figures; and in Experiment V, 14 fewer substitutions and 12 fewer impression-figures. Thus in the first four experiments not only had past experience shown its influence by a change in the number of impression-figures substituted for test-figures, but also by a change in the absolute number of impression-figures reported.

Tables II and III have been dealing with the total number of impression-figures reported and the total number of substitutions of impression- for test-figures, without regard to their distribution within the particular reports. Inasmuch as some of the test-slides contained two test-figures, for either or both of which substitutions might have been made, it is evident that the number of reports showing substitutions is not necessarily identical with the total number of substitutions made.

Consequently the difference in the number of substitutions given by the experimental and the control groups might be due either to a difference in the number of slides in which these substitutions occurred or to a difference in the frequency of substitutions within particular slides. In order to investigate this matter a tabulation was made of the number of slides showing substitutions.

In Table IV, column 1 shows the individual slides in question. Columns 2 and 3 show, for the 20 subjects of the experimental and of the control group, the number of reports containing such substitutions. Columns 4 and 5 show the number of reports with substitutions as percentages of the total number of reports. Column 6 shows the differences between these percentages for the two groups (*i.e.*, the differences between the percentages given in columns 4 and 5). It is therefore evident not only that the experimental group in Experiments I to IV has made a greater absolute number of substitutions, but that in these same experiments the substitutions have appeared in a greater number of slides.

Effect of the Type of Figure Used.—It has already been suggested that the results are in part a function of the type of figure used. The results of the five experiments will therefore be compared in order to disclose this function in detail. As Tables II and III show, both the increase in the number of impression-figures reported and the number of substitutions by the experimental group as compared with the control group were considerably greater for Experiment I than for Experiment II. The difference between the stimuli in these two experiments was the use in Experiment I of irregular quadrilaterals as impression-figures and triangles as test-figures, and in Experiment II of triangles as impression-figures and irregular quadrilaterals as test-figures—the number of figures, their arrangements on the slides, and mode of presentation were rigorously the same. The difference in results is therefore a function of the figures employed, though it is not possible at this point to ascertain the relative rôles of the figures of the impression-series and those of the test-series.

Experiments III and IV also were identical except for the

TABLE IV

NUMBER OF REPORTS IN WHICH SUBSTITUTIONS OF THE IMPRESSION-FIGURE WERE MADE FOR TEST-FIGURES IN THE TEST-SLIDES

1 Stimulus Slide	2	3	4	5	6 Difference between the Two Groups
	No. of Reports with Substitution of the Impression-Figure		Percentage of Reports Which Show Substitutions		
	Experimental Group	Control Group	Experimental Group	Control Group	
Experiment I					
No. 10	15	5			
No. 13	17	8			
No. 14	5	6			
No. 17	3	4			
No. 18	—	—			
Total	40	23	40%	23%	17%
Experiment II					
No. 10	16	10			
No. 13	17	7			
No. 14	3	3			
No. 17	11	6			
No. 18	—	—			
Total	47	26	47%	26%	21%
Experiment III					
No. 10	2	3			
No. 12	17	13			
No. 14	—	1			
No. 16	—	—			
No. 18	18	5			
No. 20	—	—			
Total	37	27	31%	18%	13%
Experiment IV					
No. 10	—	—			
No. 12	19	12			
No. 14	—	—			
No. 16	—	—			
No. 18	8	1			
No. 20	—	—			
Total	27	13	23%	10%	13%
Experiment V					
No. 10	2	5			
No. 12	19	19			
No. 14	—	—			
No. 16	—	—			
No. 18	18	16			
No. 20	—	—			
Total	39	40	33%	33%	—

types of figures used, which were squares for impression-figures with triangles and pentagons for test-figures in Experiment III, against triangles for impression-figures and squares and pentagons for test-figures in Experiment IV. Here again there is a difference between the experiments, though the small size of this difference makes its reliability uncertain.

Finally, Experiments III and V were identical, not only in general features, but also in the use of squares as impression-figures; they differed solely in the test-series, where equilateral triangles and pentagons were used in Experiment III, while in Experiment V triangles and pentagons which retained more of a square configuration (consisting of a square dot schema from which one dot had been omitted or one added) were employed. Due to the use of this square schema, the observers of the control group gave a number of reports of squares closely comparable to the number given by the observers of the experimental group. The predisposition for the perception of squares was already so great for both groups that the added experience of the experimental group was insignificant. This comparison of Experiments III and V therefore confirms the observations made above of the effect of the type of figures used.

The variations for the different experiments in the effects of the 'past experience' provided by the preceding impression-series have been seen to be in large measure dependent on the type of figure used. These differences may be formulated as based upon peculiar perceptual qualities of the particular figures. The results thus far, however, give no reliable characterization of these perceptual qualities, because of the varying combinations of figures used, the unequal number of different types of figures occurring on some of the slides, and the uncertain predispositions set up by the presentations that preceded a given slide. The following experiments were carried out in order to study under more controlled conditions the mode of perception of the figures, and the effect of using them in combination.

Reports for the Experiments Testing the Mode of Perception of Different Figures.—There were six experiments (Experiments VI to XI as shown in Fig. 2) concerned with the

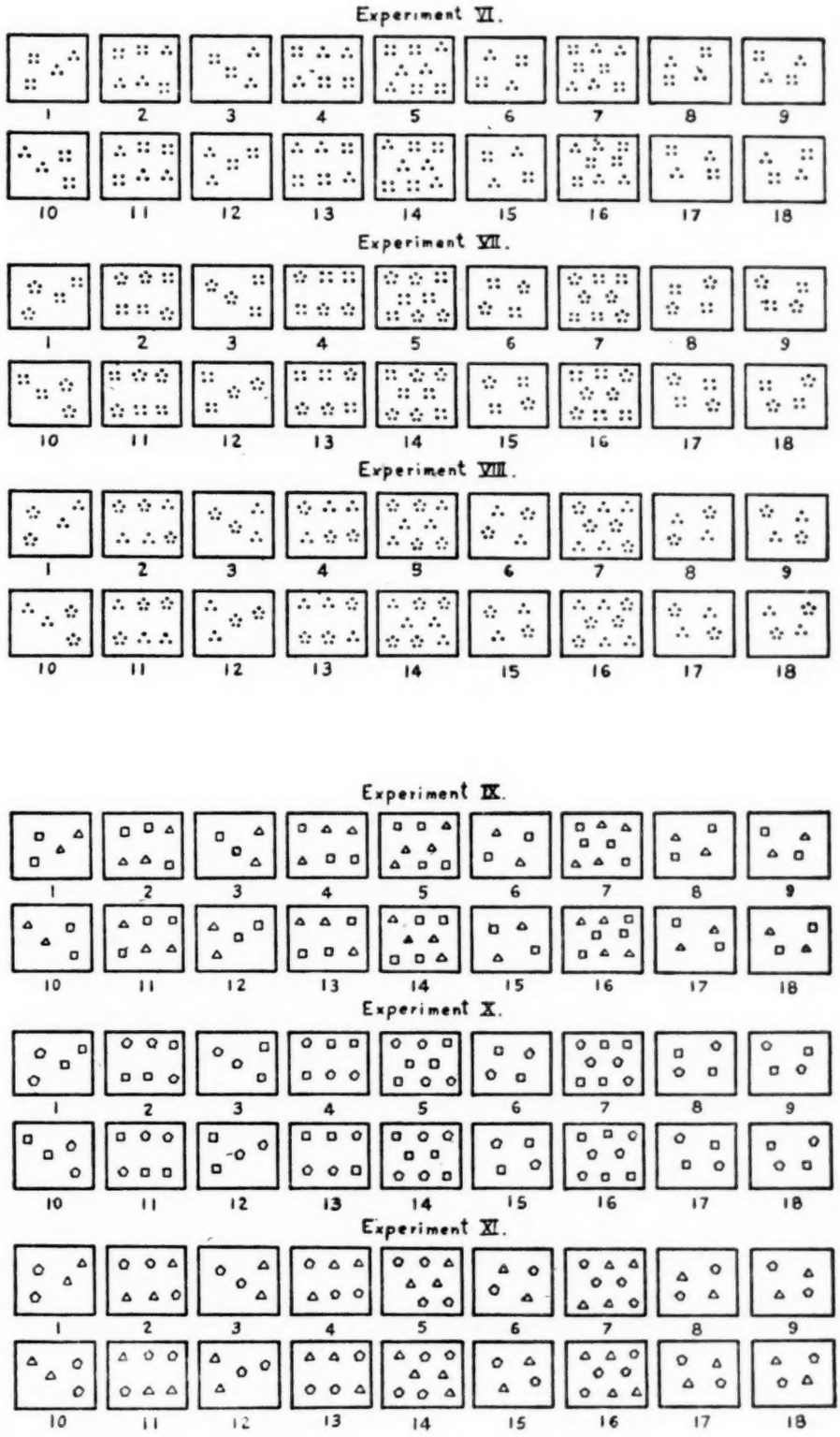
Experiment VI.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18
Experiment VII.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18
Experiment VIII.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18
Experiment IX.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18
Experiment X.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18
Experiment XI.
1 2 3 4 5 6 7 8 9
10 11 12 13 14 15 16 17 18

FIG. 2.

mode of perception of the several types of figures in question. In Experiments VI, VII, and VIII slides were used on which were dot schema representing an equal number of two types of figures; in Experiments IX, X, and XI the slides were duplicates of these except that full outline forms were substituted for the dots. In all these experiments the last nine slides were mirror images of the first nine, so that position factors should be equal for the two types of figures used. These two halves are given separately in the tables in order to disclose any practice effects. Column 1 of Table V shows the figures reported for these experiments. Column 2 gives the total number of figures of each type presented. Columns 3 and 4 indicate the number of these as well as the number of unpresented types of figures reported for the first and second halves of the experiment respectively. Column 5 gives the total numbers. Column 6 gives in percentages the relation of reported figures to those presented. In the footnotes beneath the table are given itemized classifications of the figures of the unpresented types which were reported.

Experiment VI in which squares and triangles were presented, showed the squares to be much more readily perceived, the proportions being 114 percent for squares and 66 percent for triangles. Squares are also more readily perceived when compared with pentagons, as shown in Experiment VII, the proportions being 113 percent for squares and 57 percent for pentagons. It will be noted that the number of squares reported in the two experiments is about the same, suggesting that under the conditions the perception of squares was not particularly influenced by the type of figure with which they were combined.

Experiments VI and VII showed a few more reports of triangles than of pentagons when these appeared with squares. Experiment VIII compared triangles and pentagons directly, and showed likewise a slight tendency in favor of triangles, the figures being 65 percent and 56 percent respectively. Again the results indicate that the ease of perception of a given type of figure was independent of the type of figure in combination with which it was presented.

TABLE V

NUMBER OF FIGURES REPORTED IN THE EXPERIMENTS TESTING THE EASE WITH WHICH DIFFERENT FORMS ARE PERCEIVED

1 Type of Figure	2 Total No. of Figures Presented	3 No. of Figures Reported: First Half of Experiment	4 No. of Figures Reported: Second Half of Experiment	5 No. of Figures Reported: Total	6 Percentage of Figures Reported
Experiment VI					
Squares	960	575	520	1095	114%
Triangles	960	288	349	637	66%
Others	0	0	0	0	
Experiment VII					
Squares	960	549	533	1082	113%
Pentagons	960	254	290	544	57%
Others	0	102	95	197	
Experiment VIII					
Triangles	960	269	355	624	65%
Pentagons	960	287	248	535	56%
Others	0	345	347	692	
Experiment IX					
Squares	960	470	457	927	97%
Triangles	960	438	459	897	93%
Others	0	0	0	0	
Experiment X					
Squares	960	533	508	1041	108%
Pentagons	960	286	364	650	68%
Others	0	51	34	85	
Experiment XI					
Triangles	960	434	458	892	93%
Pentagons	960	263	273	536	56%
Others	0	144	135	279	

(Experiment VII.) Unpresented figures:—99 triangles, 83 hexagons, 1 circle, and 14 unclassifiable figures.

(Experiment VIII.) Unpresented figures:—538 quadrilaterals, 37 hexagons, 16 circles, and 94 unclassifiable figures.

(Experiment X.) Unpresented figures:—28 triangles, 26 hexagons, 18 circles, and 13 unclassifiable figures.

(Experiment XI.) Unpresented figures:—187 quadrilaterals, 32 hexagons, 35 circles, and 25 unclassifiable figures.

On the contrary, the number of imaginary figures (*i.e.*, figures of types that had not been presented) does seem to be dependent upon the particular combination of figures. None of these imaginary figures was reported for Experiment VI,

but 197 were reported for Experiment VII, and 692 for Experiment VIII. Thus although in Experiment VIII nearly the same number of triangles and pentagons were reported as when each was presented with the squares, yet the number of imaginary figures was enormously increased over the number accompanying them in the other combinations. Now it will be noted that the total number of figures reported in any of the given situations is nearly the same, being 1732, 1823, and 1851 respectively for Experiments VI, VII, and VIII, and hence it appears that the frequency of reports is a function of the number of presentations and is independent, or essentially so, of the type of figures concerned. It is indicated further that the less easily perceived figures tended to give rise to fewer correct reports of themselves, and also to be less readily substituted for other figures with which they were combined. Hence the reports were filled out with imaginary figures, and this occurred less often in Experiments VI and VII because squares are easily perceived, and more often in Experiment VIII because triangles and pentagons are perceived with comparative difficulty.

The proportion of different types of imaginary figures varied for the different experiments. In Experiment VII the reports of imaginary figures included 99 triangles, 83 hexagons, 1 circle, and 14 unclassifiable figures. In Experiment VIII the imaginary figures consisted of 538 quadrilaterals, 37 hexagons, 16 circles, and 101 unclassifiable figures. To explain why these particular figures occurred involves several considerations. In the first place, it is evident here, as above, that the types of figures varied in the ease with which they were perceived. Squares were far more readily perceived than any other figures used. When other figures were presented the observer's reports indicated that often the perception meant to the observer little more than that the figure was not a square; and the result was that in his report he sketched some non-rectangular figure. The types of simple geometrical figures which are either readily apprehended or easily sketched are few in number, and hence the range of substitutions for poorly perceived figures is highly limited. In such a position,

the observer probably reports any non-rectangular figure—a triangle, hexagon, or circle—the frequency of such particular types reflecting their perceptual difficulty for him. The imaginary figures reported in Experiment VII may reasonably be regarded as mainly substitutions for the pentagons since even more squares were reported than were presented. The large number of hexagons in Experiments VII and VIII may be partly due to the fact that in a brief exposure the pentagons were rather complex, and might merely have given an impression of a 'many-sided polygon.' Also, it is easier to draw hexagons than pentagons so that some of these may have been accidentally drawn in place of pentagons, although an attempt was made to guard against such error by asking the observers after each experiment to name the types of figures seen.

With the outline stimuli of Experiments IX, X, and XI, the results are much the same. In Experiment IX, 97 percent as many squares were reported as were presented and 93 percent as many triangles. In Experiment X, the frequencies were 108 percent for squares and 68 percent for pentagons; and in Experiment XI, 93 percent for triangles and 56 percent for pentagons. Hence with an equal number of each type of figure as stimuli the number of reported figures was greatest for squares, intermediate for triangles, and least for pentagons. This is the same order of effectiveness as was shown for the stimuli represented by the dot-schema. The preponderance is not so great in favor of squares, probably because the other types were more readily perceived when in line drawings than when in dot-schema. But the hierarchy of squares, triangles, and pentagons still was maintained.

In the preceding discussion the figures have been considered without regard to their particular distribution among the various slides; it may be significant to determine whether the relative frequencies of perceived figures was a function of increased frequency of certain figures in a limited number of slides or whether they were due to a general increase of these figures in the whole array of slides. Hence in Table VI the slide-reports themselves are analyzed. In column 1 are listed

TABLE VI

TYPES OF REPORTS GIVEN IN THE EXPERIMENTS TESTING THE EASE OF PERCEPTION FOR DIFFERENT FORMS

1 Type of Report	2 No. of Reports Given: First Half of Experiment	3 No. of Reports Given: Second Half of Experiment	4 No. of Reports Given: Total	5 Percentage of Total Reports (Percent of 360)
Experiment VI				
Correct (equal no. of squares and triangles)	44	52	96	27%
Incorrect, but equal no. of squares and triangles	16	19	35	10%
More squares than triangles	115	88	203	56%
More triangles than squares	5	21	26	7%
Including other figures	0	0	0	0%
Experiment VII				
Correct (equal no. of squares and pentagons)	12	28	40	11%
Incorrect, but equal no. of squares and pentagons	15	24	39	11%
More squares than pentagons	125	109	234	65%
More pentagons than squares	28	19	47	13%
Including other figures	59	39	98	27%
Experiment VIII				
Correct (equal no. of triangles and pentagons)	7	11	18	5%
Incorrect, but equal no. of triangles and pentagons	24	32	56	16%
More triangles than pentagons	72	82	154	43%
More pentagons than triangles	60	48	108	30%
Including other figures	119	122	241	67%
Experiment IX				
Correct (equal no. of squares and triangles)	97	99	196	54%
Incorrect, but equal no. of squares and triangles	40	41	81	23%
More squares than triangles	28	21	49	14%
More triangles than squares	15	19	34	9%
Including other figures	0	0	0	0%
Experiment X				
Correct (equal no. of squares and pentagons)	53	75	128	36%
Incorrect, but equal no. of squares and pentagons	19	21	40	11%
More squares than pentagons	98	71	169	47%
More pentagons than squares	10	13	23	6%
Including other figures	38	25	63	18%
Experiment XI				
Correct (equal no. of triangles and pentagons)	31	41	72	20%
Incorrect, but equal no. of triangles and pentagons	32	29	61	17%
More triangles than pentagons	84	95	179	50%
More pentagons than triangles	28	14	42	12%
Including other figures	70	75	145	40%

the classes into which the reports for each experiment have been grouped: (*a*) correct reports; (*b*) incorrect reports which, however, gave an equal number of the two types of figures presented; (*c*), (*d*) reports in which a greater number of one of the two stimulus-figures was reported; and (*e*) reports in which figures of other than the stimulus-types were given. Column 2 shows the number of reports for the different classes for the first nine slides, *i.e.*, the first half of the experiment, while column 3 gives the number of reports for the second half; column 4 the totals for the entire experiment. Column 5 gives the percent of the total number of reports for each class.

It is evident that the number of slides reported correctly was greater for the outline stimuli (Experiments VI, VII, VIII) than for the dot stimuli (Experiments IX, X, XI). But for both dot and outline stimuli the experiments containing squares, Experiments VI and VII and Experiments IX and X, gave more correct reports than the other experiments. This suggests that the squares tended to be more accurately reproduced than the other figures. The triangles were the next accurately reproduced, and the pentagons the least. Of the slide-reports for Experiment VI, VII, and VIII which gave a predominance in the number of one type of figure over the other type presented with it, 437 showed a predominance of squares, 180 a predominance of triangles, and 155 a predominance of pentagons; of such reports for Experiments IX, X, and XI, 218 showed a predominance of squares, 213 a predominance of triangles, and 65 a predominance of pentagons.

To determine the relative difficulty of the stimulus figures the above data have been analyzed in three ways, namely, the number of different types of figures reported, the number of correct reports with the different combinations of figures, and the predominance in number in the reports of one type of figure over another. All of these methods of analysis have led to the same order of effectiveness for the stimulus-figures.

The question of a general practice-effect which would result in a difference in the reports for earlier and later members of the series is yet to be considered. The data have, however, been given separately for the first and the second

half of the experiments. Since the last nine slides were mirror-images of the first nine the position-factor was controlled, and the two parts are comparable. It may be seen from Table VI that in every experiment the number of correct reports was greater in the second half of the series than in the first half. Likewise the number of reports which though incorrect gave an equal number of the two types of figures was greater in the second half in all but the last experiment. As to the number of the different types of figures reported, Table V shows that in every experiment in which squares were used there was a decrease in the number of squares reported for the latter part of the experiments. There was, on the other hand, an increase in the number of triangles in all experiments in which they were used. The number of pentagons was also somewhat greater in three experiments out of the four in which they were used. It appears further in Table V that there is a slight reduction in the number of imaginary figures in the second half of the experiments. In short, practice, in so far as it shows any consistent trend, appears to lead to a greater accuracy of reproduction. Its effect varies according to the particular figures concerned.

The above experiments have been concerned with the relative effectiveness of different types of stimuli when an equal number of two types of figures were presented together. In the experiments testing the effects of past experience, however, there were slides in which one type of figure predominated in number over another type. Experiment XII was planned to determine if this factor gives rise to any unusual types of report.

Reports for the Experiment Testing the Effect of Predominance in Number of One Figure over Another.—The following experiment (Experiment XII) was devised to ascertain the effect of relative frequencies of squares and triangles on a given slide. The stimuli were arranged as shown in the two rows of Fig. 3. Each of the 20 slides, as shown, contained an unequal number of these two types of figures. For each type of figure there were 4 slides showing one more figure of that type than of the type with which it was combined, 3 slides

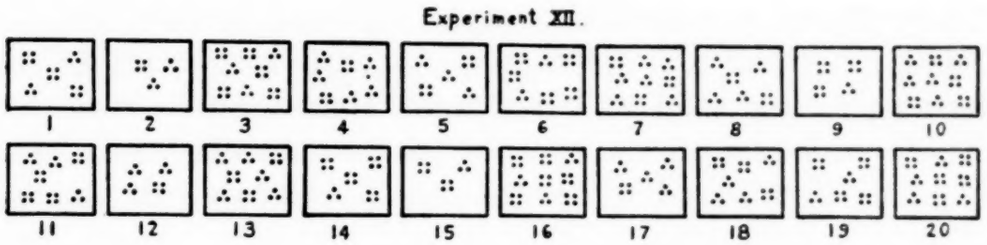

FIG. 3.

presenting two more figures of that type, and 3 presenting three more figures of that type. The number of figures on any given slide varied from 3 to 9, with the average at 6 for slides in which one type of figure predominated by one or two, and at 7 for slides in which one type of figure predominated by three. Aside from the interchanging of the types of figures, the slides showing a predominance of one figure were duplicates of those showing a predominance of the other. The two types of figures thus were balanced as regards both number and distribution.

The results of this experiment (for 20 observers as before) are shown in Table VII. Column 1 shows the classes of stimuli, grouped according to the predominance of one of the figures. Columns 2 and 3 show the total number of figures presented for the different classes. Columns 4 and 5 show

TABLE VII

NUMBER OF FIGURES REPORTED FOR THE SLIDES IN WHICH ONE TYPE OF STIMULUS FIGURE PREDOMINATED

1 Predominance of Stimuli	2	3	4	5	6	7
	Total No. Figures Presented		Total No. Figures Reported		Percentage of Figures Reported	
	Squares	Triangles	Squares	Triangles	Squares	Triangles
Squares predominated by 1	280	200	307	146	109%	73%
Triangles predominated by 1	200	280	265	172	133%	61%
Squares predominated by 2	240	120	232	93	97%	78%
Triangles predominated by 2	120	240	169	161	141%	68%
Squares predominated by 3	300	120	262	114	87%	95%
Triangles predominated by 3	120	300	200	164	167%	55%
Totals	1260	1260	1435	850		

the number of squares and triangles reported for these classes. Finally, columns 6 and 7 show for squares and triangles the percentages reported as based upon the number presented.

The ratio of squares reported was 109 percent when these figures predominated in the stimulus-slides by one, 97 percent when they predominated by two, and 87 percent when they predominated by three. The corresponding percentages for triangles were 61 when they predominated by one, 68 when they predominated by two, and 55 when they predominated by three. Thus it appears that with a single exception (predominance of triangles in the stimulus-slides by two), the number of figures reported varied inversely as the degree of predominance of that figure. When triangles predominated, by one, two, and three, respectively, the ratio of reported to presented squares was 133, 141, and 166 percent. When the squares predominated in the same way, the ratios for triangles were 73, 78, and 95 percent. Hence it appears that the greater the predominance of one type of figure, the greater the relative frequency of report of the other type of figure. But it must be kept in mind that the average number of figures per slide was kept at 6 or 7, and hence predominance of one type of stimulus entailed a decrease in the number of stimuli of the other type. Now if it can be assumed that perceptual difficulty for a given type of figure is a direct function of the number of that type presented, regardless of the presence of other types of stimuli, then plainly the increasing predominance of one type of figure in the above experiment is accompanied by a reduction of perceptual difficulty as regards the other type of figure. On this assumption may be explained the increased proportion of reports of a given type of figure with increased predominance of the other type in the stimuli presented.

Discussion and Conclusions

The results show that the perception of visual stimuli may be significantly modified by past experience resulting from the previous perception of other visual stimuli. This modification may be manifested either as changes of the form of the

figures perceived or as actual additions of new figures in the total perception.

Such modification of perception by past experience is not invariably shown, however. In one of the five experiments (Experiment V) there was no significant difference between the results of the observers who had been subjected to a foregoing impression-series and those who perceived the test-series without any such previous training. In this case, as has been pointed out, the form of the stimuli was such as to bring in from the general past experience of the observers tendencies so strong as to make negligible any tendencies which the impression-series might be expected to establish. Hence it is plain that an experimental situation may be deliberately created, as was the case in Experiment V, in which past experience will not statistically be revealed, in spite of its essential rôle in that situation.

Another manner of creating an experimental situation which tends to obscure the factor of past experience is shown in Gottschaldt's experiments,[12] in which observers were aided but little if at all in discovering a simple form geometrically present in a complex maze of lines by any previous experience with that single figure, for the very reason that their previous experience dealt with figures in simple isolation and not woven into a bewildering fabric of lines and angles and cross-hatchings.

Other conditions also may obscure the effect of past experience. Obviously, if the perceptual task is made too easy a preceding period of training will be of little importance and its effects will show little if at all. In the present experiment such was the case with the slides containing but one or two figures, for all observers tended to perceive them correctly regardless of whether a previous impression-series had been presented.

Another condition that presumably would obscure the effect of a preceding training series would be fatigue or monotony of a specific kind brought about by the presentation of a

[12] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. I. Über den Einfluss gehäufter Einprägung von Figuren auf ihre Sichbarkeit in umfassenden Konfigurationen, *Psychol. Forsch.*, 1926, **8**, 261-317.

given figure an excessive number of times. Such may very well have occurred in Gottschaldt's experiment in the presentation of the same figure as many as 520 times.

We may follow Gottschaldt [13] in making a distinction between effects specific to the particular type of stimulus employed, and what is more generally referred to as practice- and adaptation-effects, *i.e.*, effects which apply to all types of figures and not merely to the particular type used in the impression-series. It is plain that the effects of Experiments I to IV were specific to the particular impression-figures used. There was an increased number of substitutions of the impression-type of figure for the test-figures and an increased number of impression-figures reported beyond the additional ones due to the substitutions, rather than an improvement in the reproduction of all types of figures. An effect of the second sort appeared in Experiments VI to XI as a practice-effect, shown as a general increase in the accuracy of reproduction.[14] Doubtless this practice-effect was in a measure due to better adaptation to the experimental setting. It might perfectly well have appeared if entirely new figures had been substituted for the types for which there had been practice in reproduction.

The extent to which past experience showed its influence varied considerably for the different experiments, and this variation was found to be dependent upon the particular stimulus-figures used. These figures were studied, therefore, in regard to their relative difficulty of perception. The types of figures were found to vary greatly in such difficulty, and, moreover, their difficulty was found to be relatively independent of other types of figures with which they were combined. For equilateral figures the order of difficulty from least to most difficult was squares, triangles, and pentagons, judged either as to number of figures of each type reported, or degree of accuracy of reproduction.[15] Irregular quadrilaterals

[13] *Op. cit.*, 283.

[14] By accuracy of reproduction is meant the accuracy as to number and position. No attempt was made to compare the reported figures with the presented stimuli on the basis of their proportions or size.

[15] As H. Helson and E. V. Fehrer have shown, a figure may be easy ('good') according to one criterion and relatively difficult ('poor') according to another criterion.

judged by the results of the impression-series (Experiments I and II) were more difficult to perceive than triangles and probably fall between triangles and pentagons in difficulty. This same order held with outline stimuli as well as with the dot-schema, though as a class the outline stimuli were on a lower level of difficulty than the dot stimuli.

For the more difficult combinations of stimulus-figures a great many imaginary figures were reported. Among these imaginary figures the order from most to least common were quadrilaterals (including squares), triangles, hexagons, and circles. The proportions of these different figures were about the same for the two classes of stimuli, except for circles, which were considerably increased for the outline stimuli, due presumably, to the fact that in outlines the corners or angles are less outstanding than the points of a dot-schema.

This matter of difficulty has a further rôle. It will be recalled that though Experiments I and II and Experiments III and IV were comparable in that they differed only in the stimulus-figures used, yet it was impossible to known what part of the difference between the results for them was attributable to the difference in impression-figures and what to the difference in test-figures. However, the results for both the impression-series and the experiment testing the ease of perception for different figures show that the irregular quadrilaterals used as impression-figures in Experiment I were more difficult than the triangles used as impression-figures in Experiment II, and, similarly, the triangles used as impression-figures in Experiment IV were more difficult than the squares of Experiment III. Further, the test-figures of Experiments I and IV were less difficult than the corresponding test-figures of Experiments II and III. Now, the influence of the impression-series was shown to a somewhat greater extent in Experiments I and IV than in Experiments II and III, and this difference may be explained on one or both the following

They found far from a strict correlation in the rank of 'goodness' of various forms when judged; (1) by the illumination required to recognize them; (2) by the tendency for them to be reported for other forms; (3) by the tendency for other forms to be reported for them; and (4) by the tendency for them to be reported with certainty. See, The rôle of form in perception, *Amer. J. Psychol.*, 1932, **44**, 79–102.

hypotheses: that the more difficult impression-figures were relatively more influential or that the more difficult test-figures were already so frequently substituted for, without an impression-series having preceded them, that less opportunity remained for the impression-series to show its influence.

The influence of the impression-series, as evidenced by the reports of the observers, was exerted without their knowledge; there was no indication that there had been established here, as Gottschaldt reported for his experiment, an attitude of 'searching' for the impression-type of figure. There was no break or change of procedure between impression- and test-series, and hence the observer had no cue to a change of attitude that would affect one series in a manner qualitatively different from the other. Substitutions of impression- for test-figures often were made in reports following those in which test-stimuli were correctly reported, and often even in the same report with a figure of the test-type. Hence there was no attitude such as would lead the observer to disregard the stimulus-data as to the types of figures involved and attend only to their number and position.

The results of the present study show that past experience provided by the perception of certain visual forms influences significantly the perception of succeeding forms. Moreover, this influence is specific to the particular impression-figure used, though the extent to which the influence is shown depends both upon the type of impression-figure and the type of test-figure employed, since figures vary markedly in their perceptual difficulty.

Summary

(1) The presentation of a particular type of figure in a short impression-series noticeably influences the reports of succeeding figures.

(2) The effects of the impression-series are specific to the particular type of figure used in that series, the influence being shown both as an increase in the number of substitutions of impression- for test-figures, and as an increase other than substitutions in the number of reports of figures of the impression-type.

(3) Simple figures vary considerably in the difficulty with which they are perceived, as judged both by the proportion of each type reported and by the accuracy of reproduction as regards their number and position. Of the figures used, squares were the most readily perceived, while triangles and pentagons followed in the order named. Irregular quadrilaterals were more difficult than triangles and indirect comparisons indicate that they would probably rank between the triangles and pentagons in the above order.

(4) The extent to which past experience is effective varies according to the particular figures employed as impression- and test-figures, and is definitely related to the degrees of perceptual difficulty of the figures.

(Manuscript received June 8, 1933)

A STUDY OF TREMOR IN NORMAL SUBJECTS[1]

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Introduction

This is a study of the minute barely visible fluctuations of articulate members of the human body. Of these one is hardly aware except during skilled manipulations or in pathological conditions which make the movements so gross as to be disturbing.

Historically the phenomenon of tremor has long been known. Galen (1) is said to have observed it in certain pathological conditions, and to have noted that it differed during voluntary movement and repose. Perhaps the classical works on normal tremor are those of A. Eshner: A Graphic Study of Tremor, 1897, and L. Binet: Recherche sur le tremblement, 1918. Wright (2) as recently as 1931 stated that the biological significance and the 'raison d'etre' of the movements were unknown.

Instrumentation and Methods of Control

This study offers a rigid attempt at controlling the postural orientations under which registrations were made, and careful consideration of the manner of coupling the subject to the registering system.

Since the energy involved in tremor fluctuations is extremely slight, and the momentum nearly negligible, it is important to reduce any possible damping factor within the recording system to a minimum. In the present technique the phonelescope, an optical lever, was used for photographic recording. The diaphragm of the phonelescope was joined by means of high pressure tubing to a rate-of-climb indicator diaphragm[2] of the type used in an aeroplane barograph. This indicator diaphragm is so constructed that very slight vertical deformation gives large air-volume displacement.

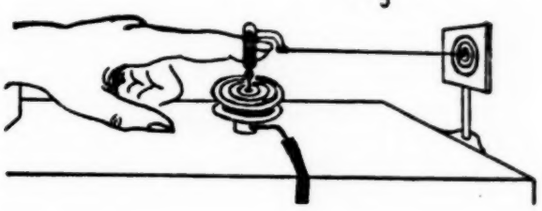
It was to this diaphragm that the fluctuating member to be studied was coupled

[1] Conducted in the University of Toronto Psychological Laboratory under the direction of Professor E. A. Bott.

[2] Procurable from the Pioneer Instrument Company, Lexington Avenue, Brooklyn, N. Y.

by means of a ball and socket joint (Fig. 1). A light firm band which applied uniform pressure around the member so diffused cutaneous stimulations as to render a minimum any possible directional clues from the diaphragm and the coupling mechanism. Introspective reports indicated that the subjects were not aware of directional kinæsthetic or cutaneous clues.

fig 1



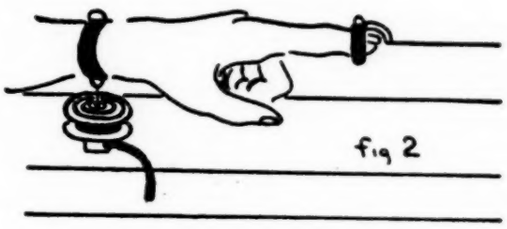
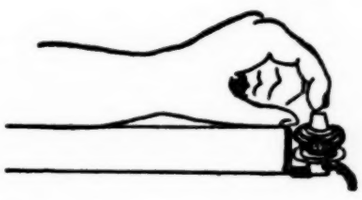


fig 2

fig 3

fig 4



The pointer and the target shown in Fig. 1 served the double purpose of providing a visual fixation point, and insuring against the possibility of the subject's 'riding' the indicator diaphragm. Additional precaution was taken, by means of pressure valves, to insure the operation of the phonelescope diaphragm well within its effective range of excursion.

The fixation of joints the movement of which was not desired was accomplished by means of suitable rests, plasticene casts, and specially taped paper cylinders.

Conditions Under Which Records Were Taken

Five adult subjects, two men and three women, were used. The observations were made in the middle or late afternoon. In every case the subjects reported nothing

in the way of activity in excess of their usual routine for a period of twenty-four hours preceding registration. Neither sex differences nor fatigue effects were the object of study, but rather the characteristics of tremor in adults under normal conditions.

Records were taken from the index finger or the forearm with different articulations active, with either kinæsthetic clues alone or kinæsthetic plus visual clues, with relaxed muscular tension, normal tension, or voluntarily increased tension. For an outline of the plan of experimentation see Table I.

TABLE I

PLAN OF EXPERIMENTATION

(Read across)

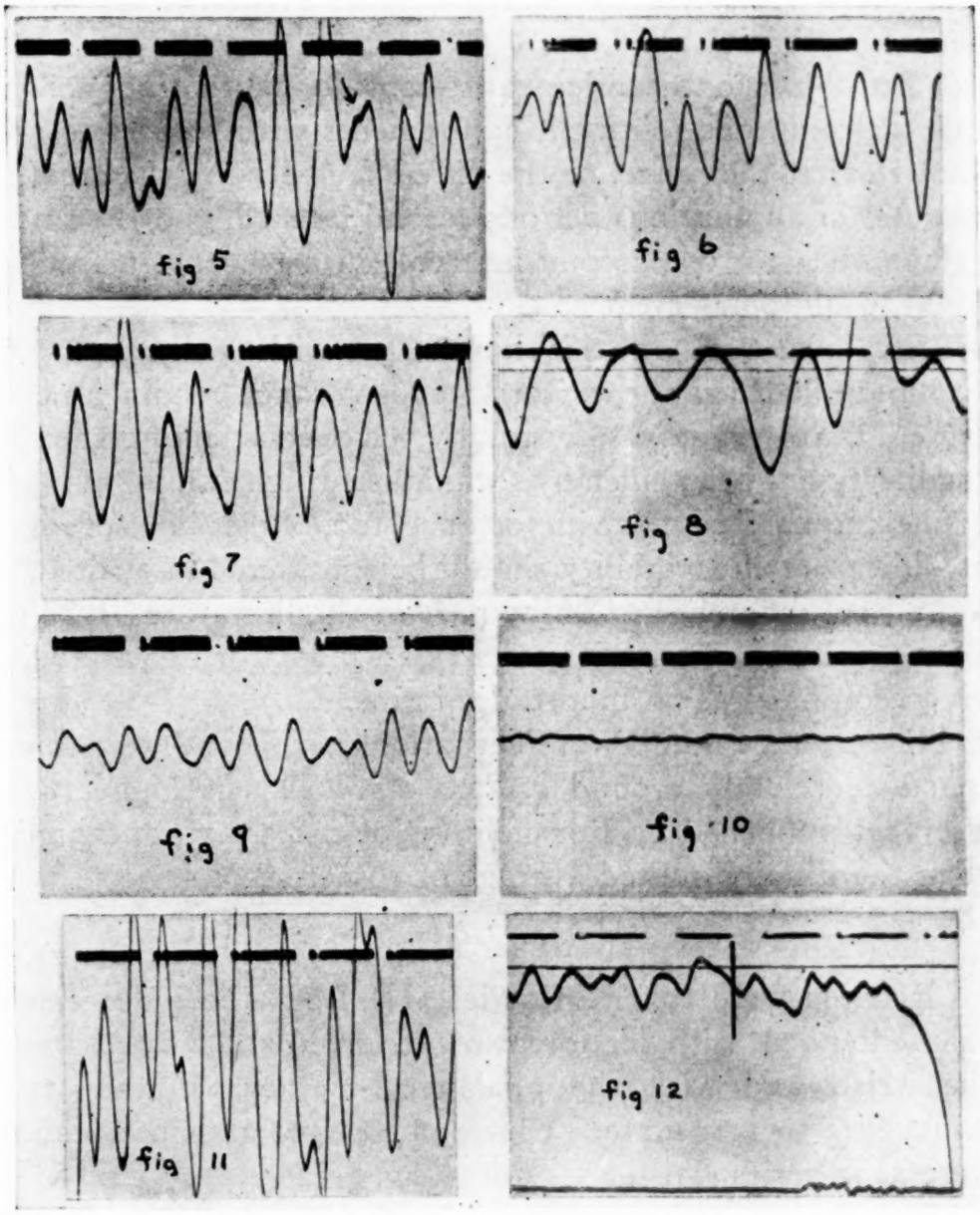
Series	Records Taken from	Fluctuating Members	Active Articulations	Sensory Controls
I	Tip of index finger.	Index finger (taped)	Knuckle*	Kin. + vis. (target)
II	Tip of index finger.	Finger and hand	Knuckle* + wrist	Kin. + vis. (target)
III	Tip of index finger	Finger, hand, forearm, upper arm	Knuckle,* wrist, elbow, shoulder	Kin. + vis. (target)
IV	Distal end of forearm.....	Forearm, upper arm	Elbow, shoulder	Kin. + vis. (target)
V	Tip of index finger (*cf.* II).....	Finger and hand	Knuckle* + wrist	Kinæsthetic
VI	Tip of index finger (flexed).....	Finger (flexed)	Knuckle*	Kin. + vis. (no (target)
VII	Tip of index finger (*cf.* II).....	Finger and hand (vol. tension)	Knuckle* + wrist	Kin. + vis. (target)
VIII	Tip of index finger (before voluntary movement).	Index finger (taped)	Knuckle*	Kin., vis., and auditory

* Knuckle or phalangeal-metacarpal.

METHOD OF MEASUREMENT

Figure 5 serves to illustrate the conventions of measurement. Reading the section of record shown from left to right all fluctuations giving an unquestioned change of slope from a positive through a zero to a negative value were counted as tremors. If as is indicated, for example, in the section designated by the arrow there was not an unquestioned change through a zero to a negative value the variation was counted merely as a modification within a single tremor oscillation.

The time line was interrupted every one fifth second. Most of the recordings were over ten second periods. The recording paper passed through the camera at a speed of c. 97 mm. per second. Dividers were used to check the constancy of this speed in sections of record used to study the relation of extent to duration of tremor fluctuations.



Series (I)

Records were taken in series (I) from the index finger. The only articulation active was the phalangeal-metacarpal. The wrist joint was fixated by means of a plasticene cast, the finger joints taped in tightly wrapped paper cylinders. The forearm was kept in the supporting rest shown in Fig. 3. Kinæsthetic and visual cues were allowed, using the target and finger pointer as described above. The instructions were: "Do your best to hold the pointer on the center of the target."

The subject assumed the degree of effort natural to him under such an instruction.

The object in this series was to consider fluctuations around one and only one joint. Measurements with respect to rate showed that the range, or the difference between the greatest number of fluctuations per one second interval and the smallest number per one second interval (in a record ten seconds long), could be as great as 5.0 d.v.

Previous work in this laboratory with a kymographic technique had led the writer to expect range readings never larger than 1.9 d.v. A totally erroneous impression of regularity has been given by techniques which damp out much of the fluctuation. The importance, too, of measures of range as indicators of variability should be considered in evaluating individual differences. In no part of the literature, in so far as the writer is aware, is range as an indicator of variability given emphasis as of importance in itself.

The average number of fluctuations per second were found to cluster closely around c. 12.0 d.v. This is higher than averages published by Bousfield (**3**) of c. 8.5 d.v., and higher than the usually quoted average of 10 per second.

Series (II)

Records were taken in series (II) from the index finger fully extended, with the forearm supported and fixated above the wrist, and with the phalangeal-metacarpal and wrist joints free to articulate. Clues and target and instructions were as in series (I).

The object here was to consider the effect of the addition of the wrist joint. The average number of fluctuations per second and the range readings (over 30 one second measures) were as follows:

Subject	Average d.v.'s per sec.	Sigma	Range
A	12.36	1.10	4.9
B	11.38	1.05	3.7
C	12.58	1.52	4.4
D	11.88	.59	2.2
E	11.79	1.14	4.0

(Original measures were accurate to one-tenth.)

The phalangeal-metacarpal and wrist joints are evidently very closely coördinated with respect to rate of fluctuation. The mean for the averages of the group is 11.99 d.v. per second. See Fig. 6.

Series (III)

Records were taken in Series (III) from the index finger as in (II) but with the arm extended. The articulations active were the phalangeal-metacarpal, wrist, elbow, and shoulder joints. The subject otherwise rested comfortably, his back against a support. Again both visual and kinæsthetic cues and the target were used. The instructions were the same as those for series (I).

The addition of the elbow and shoulder joints seemed not greatly to affect the tremor rate of the index finger. What changes there were, however, were of slight decrease. The

Subject	Average d.v.'s per sec.	Sigma	Range
A	11.08	1.28	3.9
B	10.52	.65	2.6
C	11.07	1.00	3.7
D	10.11	1.25	4.7
E	11.45	.79	2.9

mean for the averages in this series is 10.84 d.v. (*cf.* 11.99 d.v. for series II). There was in every case an increase in amplitude of which the section of record shown in Fig. 7 is typical. Figure 6 and Fig. 7 were taken from the same subject with identical adjustments of the recording mechanism.

Series (IV)

Records were taken in series (IV) from the forearm. Coupling of the arm to the indicator diaphragm was just proximal to the wrist. The articulations concerned were the elbow and shoulder joints. Again visual and kinæsthetic cues and the target were used (Fig. 2). Instructions were as in series (I).

This series throws still further light upon the mode of fluctuation of a complex of articulating segments. The postural orientation here was just the same as that for the record-

ings in series (III). The point of application to the recording system was at the wrist rather than at the tip of the index finger. See Fig. 2 and Fig. 8. The mean for the averages

Subject	Average d.v.'s per sec.	Sigma	Range
B	5.54	.78	1.9
C	6.71	.80	3.5
D	6.60	.87	3.5

in this series is 6.28 d.v. (*cf.* 10.84 d.v. for series III), almost half the average rate of fluctuation common to the index finger (*cf.* 11.99 d.v. for series II).

Adjustments of the recording system giving excursions on the recording paper of 2 to 3 centimeters in series (III) were found to give excursions for tremor at the distal end of the forearm of easily ten times this amount. This difference in amplitude may possibly be caused by a differential damping effect of the indicating diaphragm at the two points of application (end of index finger and wrist) as a consequence of the difference in the effective masses involved. But that the pressures exerted at the distal end of the forearm, in such a situation, are much greater than those exerted at the end of the index finger can hardly be exaggerated.

A further elaboration of the technique providing simultaneous registration of tremor at these two points would enable consideration of the phase relations of the fluctuations. Measurements, too, should be in terms of angular displacement as well as vertical displacement.

Series (V)

Records were taken in series (V) from the index finger with the situation exactly as in series (II) except that the subject kept his eyes closed. The instructions were: "Center your pointer on the target and then close your eyes while the record is being taken."

The object in this series was to compare registrations made when the subject had available only kinæsthetic cues with registrations in which both visual and kinæsthetic cues were

Subject	Average d.v.'s per sec.	Sigma	Range
A	10.67	1.06	3.8
B	10.72	.69	2.7
C	10.91	1.29	4.4
D	11.78	.70	2.9
E	10.68	.98	3.5

used. The mean for the averages in this series is 10.95 d.v. (*cf.* 11.99 d.v. for series II). For three out of five, subjects A, C, and E, the drop from the readings in series (II) was at least one cycle per second.

There was, too, an obvious drop in amplitude for three of the subjects. This change is represented in Fig. 9. Figures 6, 7, and 9 are all of subject B, and all were taken with identical adjustments of the recording mechanism.

A possible explanation of the decrease with respect to amplitude and rate of fluctuation may be found in the suggestion that the subject was less on the 'qui vive' when his eyes were closed. One subject reported himself to be less 'on a tension,' another less 'keyed up,' another more 'relaxed' when eyes were closed.

Not being able to use a visual check on errors of fixation the subject probably was less 'keyed up' to the work of maintaining fixation. But the unusual result was that the fixation was thereby bettered. Work under this series would indicate that postural stability with respect to tremor for both amplitude and rate of fluctuation is decreased as the introspective strain or effort increases—though with rate only minutely so. This should suggest reinterpretation of steadiness-board tests of motor skill. The results indicate further that with respect to tremor the presence of a visual cue adds nothing to kinæsthetic control of the efferent end of the neural arc.

Series (VI)

Records were taken in series (VI) from the index finger in a naturally flexed position. Visual and kinæsthetic cues were allowed, but no pointer or target. The instructions were: "Allow your hand and arm to relax on the supporting rests." Then the indicator diaphragm with cork boss affixed was brought up beneath the finger as shown in Fig. 4.

These records were taken in an attempt to get at the question of the relation of muscular tension to tremor. A typical record for the posture shown in Fig. 4 is given in Fig. 10. The amplitude of tremor in this almost completely supported passive state was so small as to render a count of the fluctuations very uncertain. But that the fluctuations were there is evident. A still more sensitive technique than the one used might reveal them countably.

Series (VII)

Records were taken in series (VII) from the extended index finger, and with target, clues, and supporting rests as in series (II). The muscular tensions obtaining up to this series have been those natural to the subjects under the particular conditions. Here the instructions were: "Tense the muscles of your hand and arm, and keeping them tense address the center of the target as well as the tense conditions permit." The conditions of this series are at the other extreme from the relaxed conditions for series (VI). The mean for the averages in this series is 12.90 d.v. (*cf.* 11.99 d.v. for Series II).

Subject	Average d.v.'s per sec.	Sigma	Range
A	12.66	1.38	4.8
C	12.62	1.51	4.8
E	13.43	.88	2.7

The amplitude of fluctuations was very much increased. See Fig. 11.

In general, then, voluntarily increasing the muscular tension increases the tremor amplitude but results in only a slight rise in the average rate of fluctuation.

The records taken in series (VI) can be counted only as normally tonic, not hypotonic in the clinical sense, certainly not atonic, so nothing can be said on the basis of these records as to what would happen in a completely atonic state—if indeed such a state can be secured without destroying the capacity for movement of a limb. The suggestion of the records in series (VI) and in series (VII) is that so long as

tonic innervation keeps antagonistic musculatures in even relative equilibrium the phenomenon of tremor fluctuations will be present, and further that the higher the degree of tonic innervation the greater will be the amplitude of the fluctuations. A measure of tremor amplitude may possibly be made to serve as an index of tonicity for the musculature in the member studied.

An astonishing constancy, for the same member, of average rates of fluctuation was noted throughout the entire study. It is certainly of vital importance to take account of this constancy in the consideration of a possible 'pace-making' mechanism behind the tremor phenomenon.

Series (VIII)

Records were taken in series (VIII) from the index finger with rests and fixation as in series (I). Recordings were made just preceding voluntary movements of extension or flexion. The subject was told: "Address your pointer to the target as previously. You will be given two auditory signals, the first to be a preparatory signal and the second to be the starting signal for a movement of extension (or flexion)." The auditory signal was the click of the key in the circuit of the signal marker.

It was the intention of the experimenter in this series to see if there was anything that might be universally characteristic of tremor following a preparatory signal, and if there was any invariable relation obtaining between the direction of involuntary tremor fluctuations and the point of inception of a voluntary movement.

In eleven out of twelve cases there was a change of rate of fluctuation following a preparatory signal. The change was sometimes of increase, sometimes of decrease. But the modification was always within a variability range of 0.0 d.v. to about 4.0 d.v.—and this is well within the range of variability found for series (I). With some of the subjects on the first one or two records there was an increase in amplitude. The transient increase was probably due to heightened emotional tone incidental to the beginning of an unfamiliar task.

Measurements made in a section of record following a preparatory signal and bearing upon the second question showed the impossibility at the present stage of the problem of predicting successfully in any given instance either the point of appearance or the duration of any given tremor fluctuation. An excursion of 18.8 mm on the recording paper might require, as in the case of subject B in one instance it did, a duration of 3.2 mm of film length, and in another case an excursion of 7.6 mm would require a duration of 3.5 mm of film length.

The following measurements showing again the variability in the relation of amplitude and duration for subject D were made in a section of record preceding a preparatory signal. Duration is again estimated in millimeters of film length—95 mm being equivalent to one second.

Single Vibration	Extent of S.V.	Duration of S.V.
1	5.0 mm	3.0 mm
2	8.2	3.6
3	3.6	3.5
4	2.0*	3.6*
5	6.9*	3.0*
6	7.0	4.0
7	11.0	5.8
8	1.0*	2.6*
9	3.0*	2.0*
10	7.9	4.5
11	10.0	5.5
12	13.4	5.5
13	11.0	5.5
14	10.0	5.6
15	5.0	3.4
16	6.9	3.6
17	6.9	4.5
18	2.0*	3.5*
19	4.2	2.5
20	7.0*	3.5*
21	8.5	8.5

(Original measures accurate to 0.2 mm.)

Note particularly the measures marked with asterisks. These are obviously in contradiction of Eshner's (4) contention that there is an inverse relation between rate and amplitude of fluctuation.

The linear coefficient of correlation, however, between extent and duration of excursion for subject D, as determined by measurements of a section of record where the only articu-

lation active was the phalangeal-metacarpal, was .824 ± .042. For subject E, as determined by measurements of a section of record where the active articulations were phalangeal-metacarpal, wrist, elbow, and shoulder, the coefficient was .596 ± .087. For subject B, as determined by measurements of a section of record following the preparatory signal and before the starting signal where only the phalangeal-metacarpal joint was active, the coefficient was .708 ± .069. It thus appears that while from fluctuation to fluctuation no invariable prediction is possible as to the extent or duration of any given tremor fluctuation over a longer period of time there is a relation bearing in the direction of Eshner's contention.

Such statistical treatment does not, however, enable us to make a prediction as to the point of inception of a voluntary movement upon the involuntary tremor movements. Travis (5) refers to certain instances in which it appears that a sudden voluntary movement may interrupt tremor apparently in any phase of its fluctuation. A record taken in this study revealing the phenomenon to which he refers may be seen in Fig. 12. One might conclude from such a record and from the measurements in series (VIII) that the mechanism of tremor is less autonomous, the phenomenon more variable than has been previously supposed. Tremor, if a low motor center phenomenon, must necessarily be in a labile and easily modified equilibrium with higher centers.

Conclusions

The measurements significant for individual differences in normal adults will probably be not in terms of average rates of fluctuation but in terms of either range of rate variation or of amplitude of fluctuation.

The better control of pressures exerted at the end of the index finger is evidently secured by the coördination of phalangeal-metacarpal and wrist joints in such a manner as to compensate for the lesser control at the distal end of the forearm.

Visual cues add nothing with respect to tremor to kinæsthetic control of the efferent end of the neural arc. The addi-

tion of a visual check actually operates in such a way as sometimes to introduce conflict or an emotional factor which increases the amplitude of fluctuation.

In general, the greater the strain or effort in maintaining 'fixation' the less the success. This suggests the necessity for standardization of procedure in 'steadiness-board' tests of motor skill. As long as tonic innervation keeps antagonistic muscular tensions in even relative equilibrium tremor fluctuations will be present, and the higher the degree of muscular tensions the greater the amplitude of the fluctuations. Variations in muscular tension will affect but little the rate of fluctuation.

What changes in tremor follow, too, as a consequence of emotional modification will be largely with respect to amplitude and only minutely with respect to rate.

Inquiry into the question of the relation of the point of inception of a voluntary movement to the phase of fluctuation of involuntary tremor movements proved the impossibility of predicting at the present stage of the problem either the point of appearance or the duration of any given tremor oscillation.

The mechanism of tremor is less autonomous and the phenomenon more variable than has been previously supposed. Tremor, if a low motor center phenomenon, is in a labile and easily modified equilibrium with higher centers.

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(3) Bousfield, W. A., The influence of fatigue on tremor, *Journ. of Exp. Psychol.*, **15**, no. 1, 1932, p. 104.
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THE RELATION BETWEEN LENGTH AND DIFFICULTY IN MOTOR LEARNING; A COMPARISON WITH VERBAL LEARNING

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The object of this investigation was to find what relationship exists between length and difficulty of material in serial motor learning and to compare this relationship with findings in the verbal field. Three hundred five (305) subjects, in 18 different groups, learned 13 mazes. Five groups learned the same 5 mazes that 5 other groups learned, but with a fuller set of instructions. Seven different lengths and 3 different types of mazes and 2 sets of instructions varying in degree of thoroughness were used.

Experiments in the verbal field conducted by Ebbinghaus,[1] Meumann,[2] Binet and Henri,[3] Henmon,[4] Lyon,[5] and Robinson and Heron [6] indicate that longer lists of nonsense syllables or numbers require more repetitions for memorizing than do shorter ones. But there is some disagreement as to the rate at which repetitions must be added with additions to the length of the material. Most experimenters find that early increases in length are accompanied by more marked increases in repetitions than are later increases in length, and that there is an early increase in number of repetitions *per syllable* necessary for learning followed by a later decrease. Robinson and Heron, however, who have done the latest piece of work,

[1] Garrett, Henry E., *Great Experiments in Psychology*, Chap. 3, Ebbinghaus's studies in memory and forgetting.

[2] Quoted by Henmon and by Robinson and Heron.

[3] Quoted by Henmon.

[4] Henmon, V. A. C., The relation between learning and retention and amount to be learned, *J. Exper. Psychol.*, 1917, **2**, 476–484.

[5] Lyon, D. O., *Memory and the Learning Process*, 1917.

[6] Robinson, E. S. and Heron, W. T., Results of variations in length of memorized material, *J. Exper. Psychol.*, 1922, **5**, 428–448.

get no later decrease. Using 10 subjects and lists of 6, 9, 12, 15 and 18 nonsense syllables, they get results which show not only a continuous increase in repetitions with increasing length, but also a continuous increase in the repetitions per syllable necessary for learning.

Warden and Hamilton [7] were the first to attempt a similar study in the motor field. They used white rats as subjects and the maze as an instrument. They, however, did not use the conventional maze, but instead the Warner-Warden 10-alley bilaterally symmetrical maze with alternating right and left turns. This particular type of maze was used, according to the authors, in order that the length of the task might be varied 'in a purely quantitative manner without introducing other variable conditions such as pattern complexity in a qualitative sense.' They believed that the rats got no cues in regard to alternating turns, since human subjects got no cues from a stylus maze of similar pattern until the maze had been completely mastered. It was suggested that the forked cul-de-sac introduced a complication which masked the relative simplicity of the pattern. With such a maze in 2, 4, 6, 8 and 10-alley lengths, Warden and Hamilton found that in most cases an increase in length did cause an increase in difficulty (6-alley length was an exception), but the increase in difficulty was not proportional to the increase in length. In other words, the difficulty of the maze per cul-de-sac decreased with an increase in length of maze or number of culs-de-sac. This is not only contrary to Robinson and Heron's findings, but to all the findings in the verbal field, since there is a continuous decrease in difficulty per unit rather than an early increase followed by a later decrease.

Allison [8] has duplicated in high relief form the Warner-Warden bilaterally symmetrical maze and has tested human subjects on lengths of 4, 6, 8 and 10 alleys, using 20 subjects to a maze. His results are strikingly similar to Warden and Hamilton's, and he concludes that this maze in high relief

[7] Warden, C. J. and Hamilton, E. L., The effect of variations in length of maze pattern upon rate of fixation in the white rat, *J. Genet. Psychol.*, 1929, **36**, 229–238.

[8] Allison, L. W., The effect of variation in length of a high-relief finger maze upon rate of learning, *J. Genet. Psychol.*, 1931, **39**, 403–407.

form does not present a comparable situation to that of the memorization of serial language material, since each additional unit to the length of the maze adds the same kind but not a comparable unit, whereas in the case of nonsense syllables each additional syllable adds a comparable but different unit.

Experimental Procedure

I. *First Part.*—In the first part of the present experiment the Warner-Warden human stylus maze in 8-alley length was duplicated in exact dimensions both in stylus and in high relief form. Allison's maze, which was found to be only an approximation of the Warner-Warden maze, was also duplicated as nearly correctly as possible in high relief form in 4 and 8-alley lengths.

Instructions to Subjects

The following, which is the simpler of the two sets of instructions, was used in both parts of the experiment except where otherwise indicated.

Read these instructions carefully. A maze is a rather intricate set of pathways leading from one point to another. The pathway in this maze is constructed by mounting wire [9] flat on the surface of a board. You are to learn the maze by tracing along the wire with the end of your index finger. The correct pathway will lead, by a more or less crooked route, from the starting point (near [10] one edge of the board) to the goal (near [10] the opposite edge). At several places along this path there will be intersecting points where it will be possible to choose the wrong path. If [11] you do this your finger will strike an obstruction (a staple driven down over the wire), and you are to trace back to, *but not across*, the intersecting point where you will choose another path. The [12] direction of your movement will be more or less forward (not straight ahead) except when retracing out of a blind.

The experimenter will put your finger on the starting point, and at the signal "all right, Start" you are to start and continue, regardless of how many errors you make, until you reach the goal. The goal is a piece of rubber tubing fastened on the board.

1. You are to strive for an early mastery of the maze, that is, to learn the path from the entrance to the goal as quickly as possible. Accuracy, however, should not be sacrificed for speed.

2. The maze will be considered learned when the subject has made five runs in succession without error (five perfect runs).

3. An error is defined as a turn from the correct path into a blind.

4. Time will be counted from the starting signal until the exit is reached.

5. An interval of about one minute will be given between trials.

6. Subjects are not to discuss the mazes or anything about the maze experiment with one another.

Cautions.—1. Do not press hard on the wire. If you do, the end of your finger

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[9] The instructions for the stylus maze were necessarily somewhat different in several places.

[10] Omitted from instructions for alternating mazes.

[11] Differently worded for alternating and U-type mazes.

[12] Omitted from instructions for alternating and U-type mazes.

may become so numb that you cannot feel the intersecting wires. It may also become tender.

2. The maze cannot be learned in one run. In fact, you are required to have a detailed and exact knowledge of the maze before it is considered learned, and little can be accomplished in one run. You should attack the maze patiently and systematically, learning a little each run if possible.

3. The maze is not a mental test and, while its solution does require concentration you should not take the attitude toward it that you would toward any kind of test. Instead you should tackle it as you would a puzzle or problem in your own room. Your grade in psychology is not affected by your maze score and this score, whether high or low, is equally valuable to the experimenter.

These instructions must be fuller than Allison's, since his subjects required much more time and many more trials and made more errors than ours. The present results, however, agree with his general findings. That is, the longer maze is slightly easier per unit than the shorter one. It was found, however, that most of the 50 S's who learned one of these two mazes or one of the two Warner-Warden type realized by or before the time they had made a perfect run that the turns alternated. This was evident not only from the testimony of the subjects but also from the following observations: (1) Some S's, after making errors on the first few alleys, got an idea of the structure of the maze and ran the rest of it perfectly. (2) Many subjects on their first exploratory trial adopted the plan of pushing as nearly straight ahead as possible. Several such subjects ran these mazes without error. (3) Most subjects after finding what the general direction to the goal was, attempted on their second trial to follow that direction, pushing more or less straight ahead, and on that trial or the next one, ran the maze correctly. For all of the above types of S's the intersecting points in the maze are not real choice points. The chances are greatly in favor of the correct pathway being entered, since the alleys turn aside from the general forward direction. This is as true of the stylus as of the high relief form when learned under the present set of instructions. There were only 3 or 4 S's who seemed to have gotten no cues from the alternating turns, and these few may have unconsciously profited from a rhythm which can be developed in alternating mazes. We, then, are in agreement with Allison in that this type of maze does not present a

comparable situation to that of the memorization of nonsense material.

Dashiell [13] has found that rats will learn mazes more easily in which there are no sharp turns or radical changes in direction. He calls this 'the forward going tendency' of rats. Rats, when offered a choice of two paths, tend to take the one which is nearer the general direction in which they have been traveling. It is also possible that rats might develop a rhythm in running an alternating maze. If so, this would question Warden and Hamilton's assumption that the rats got no cues from the alternating turns, and together with the forward-going tendency might account for the decrease in difficulty per unit of the longer mazes, since the rhythm and the forward-going tendency would have a greater chance to become established and to function in the longer mazes. This means, then, that these bilaterally symmetrical mazes are not free from qualitative differences. If exploring the first few units of a maze gives one the proper set for running the last few units correctly, all the units of the maze are not qualitatively the same. This maze, then, is probably unsuitable for rats as well as for human beings in testing the relationship between length and difficulty of material in the motor field.

II. *Main Experiment.*—For the above reasons it was decided in making this study, to use two types of mazes which are more nearly conventional. The two types are Miles's [14] X-type and Warden's [15] U-type, both in high relief form. The X-type was used in 4, 5, 6, 8 and 10-alley lengths and the U-type in 6, 10, 12 and 15-alley lengths. The 8-alley X-type maze was built by putting together two 4-alley lengths. The 12-alley U-type maze was also built by putting together two 6-alley lengths. The 10-alley X-type was constructed by adding to the 5-alley length the mirror reversal of this length. These three pairs were constructed in this manner in order to

[13] Dashiell, J. F., Direction orientation in maze running by the white rat, *Comp Psych. Mon.*, 1930, **7**, No. 2, 60.

[14] Miles, Walter R., Unidirectional mazes for studying human learning, *J. Gen. Psychol.*, 1931, **5**, 269–275.

[15] Warden, C. J., Primacy and recency as factors in cul-de-sac elimination on a stylus maze, *J. Exper. Psychol.*, 1924, **7**, 98–116.

be sure that the units used in the last halves of the longer mazes were at least objectively comparable to the units in the first halves. The longer mazes were the sums of two lengths of the shorter mazes. There is very little evidence, however, that the large mazes are subjectively the sum of two lengths of the small ones. In the case of the 6 and 12 pair, the moves are so arranged that in learning the large maze in a verbal manner, one would logically make a break between the two parts. One S in the more fully instructed group reported noticing this duplication.

At first it was intended to compare only the shorter with the longer maze in each pair. Further comparisons, however, seemed possible, so two intermediate mazes, the 6-alley X-type and the 10-alley U-type were introduced.

The 5 and 10-alley X-type and all four of the U-type mazes, a 15-alley maze being added to the 6, 10 and 12, were also learned with fuller instructions by six groups of subjects. The additional instructions consisted, (1) in showing the S a small maze of similar structure to the one he was to learn and informing him to this effect; (2) in telling him to learn the maze in a verbal manner, that is, to *memorize* and *group* the successive moves in one direction. When a person has learned a maze in this manner he has a series of numbers in mind such as 2-1-3-2, etc., corresponding to the pattern of the maze. If one uses this scheme it is not necessary to remember rights and lefts, since a break in the numbering means a change in direction.

The objection which Warden and Hamilton [16] mention might be raised here, however: that the difference between a 4 and a 5-alley maze is not only quantitative but also qualitative. That is, there is such a thing as 'pattern complexity in a qualitative sense' which would cause two mazes of similar unit structure and length but of different pattern to vary in difficulty. The number of successive moves in one direction and the number of changes in direction are two factors which have been mentioned as affecting the pattern complexity and the difficulty of mazes. We believe that these are factors of importance, but that they are not important enough in them-

[16] Warden and Hamilton, *op. cit.*

selves to displace any maze more than slightly from its proper place on the progression curve. That is, with other factors held constant, it would not displace a 6-alley maze, for instance, to such an extent that it would become easier than a 5 or more difficult than a 7-alley maze of the same type. This statement would, of course, have to be qualified to such an extent that certain obviously easy patterns are ruled out. The following are some such patterns for a 6-alley maze: alternating turns such as Warden's, all turns to left, all turns to right, and possibly first 2 to right, second 2 to left and last 2 to right, first 3 to right and last 3 to left and opposite, or first to right, second 2 to left and last 3 to right and opposite. Avoiding these and similar obviously easy patterns, other combinations of a 6-alley maze would vary very little in difficulty. We have no proof of this since all other possible combinations have not been tried. The results of this investigation, however, would suggest that it is true, since the difficulty accompanying the increase in length of any one type of maze increases in a rather definite quantitative manner. Of the 14 groups learning conventional mazes (4 of the 18 groups learned Allison's and Warden's mazes), there was only one exception of any importance to the above conclusion.

The pattern and the number of directions used in each of the 9 conventional mazes were largely matters of chance, we being interested primarily in length and in avoiding the obviously easy patterns. They are given in Table I.

It will be noticed that the number of directions in the 4, 5 and 6-alley mazes of X-type are the same, also that there is very little difference in the patterns, yet there is a decided difference in difficulty, as can be seen in Tables II, III and IV and Figs. 1, 2 and 3. In fact, objectively at least, length seems to be the only factor of importance in these 3 mazes. Length, type, and degree of instruction, in addition to individual variation among subjects, seem to be the only factors of importance with all 13 mazes and in all 18 groups. Qualitative differences, then, most probably are present, but they are of minor relative importance. It should be remembered too, as has already been mentioned, that even Warden's

TABLE I

	No. of Alleys	Pattern	No. of Directions
X-type	4	1(r)−2−1	3
	5	1(r)−3−1 and opposite	3
	6	2(l)−3−1	3
	8	1(r)−2−2−2−1	5
	10	1(r)−3−1−1−3−1 and opposite	6
U-type	6	2(l)−2−1−1	4
	10	1(r)−3−2−1−2−1	6
	12	2(l)−2−1−1−2−2−1−1	8
	15-A	2(l)−1−2−3−1−1−3−2	8
	15-B	1(r)−3−2−1−2−3−1−1−1	9

Note.—The first correct move in the 4-alley maze is to the right as indicated by the 'r' in parenthesis following the figure 1 which represents the number of moves in that direction. The next 2 moves are to the left and the last one to the right. The word 'opposite' means that half of the subjects listed as learning these two mazes learned the mirror reversal of them. The results have been combined with those of the listed mazes, since there were no significant differences between them. The results for the two 15-alley mazes have also been combined for the same reason.

bilaterally symmetrical maze, which was supposed to be so constructed as to avoid qualitative differences, probably does not avoid them.

Results

The results of this study are given in terms of trials in Table II.

On the extreme left side of the table the type of maze is indicated. The first 5 are of X-type and the next 3 of U-type. These mazes were learned with the simpler set of instructions. The next two groups of mazes, 2 X-type and 3 of the 4 U-type are the same as the above, but were learned with the fuller set of instructions. The 15-alley U-type length was also learned with the fuller set of instructions. The last four mazes are of the alternating type. The first two are Allison's, the third Warden's in wire form and the fourth Warden's in stylus form. In the next column the number of S's learning each maze is given, in the third the lengths of the mazes are indicated in terms of culs-de-sac, and in the next three the mean number of trials, the mean deviations, and the coefficients of variability, respectively, are given.

The sixth, seventh and eighth columns in Table II, headed 'percent 4 cul-de-sac,' etc., offer numerical evidence that

TABLE II

TRIALS

	No. S's	No. Cul-de-sac	Mean	M.D.	V.	Percent 4 Cul-de-sac	Percent 5 Cul-de-sac	Percent 6 Cul-de-sac	Score per Cul-de-sac
	15	4	5.67	1.82	32	100			1.42
	20	5	10.40	5.68	55	183	100		2.08
X-type	15	6	11.20	4.64	41	198			1.87
	15	8	16.47	11.42	69	290			2.06
	30	10	23.77	14.33	60	419	229		2.38
	15	6	10.47	4.20	40			100	1.75
U-type	50	10	18.00	9.32	52			172	1.80
	15	12	21.33	10.57	50			204	1.78
Inst	10	5	3.6	1.12	31		100		.72
X-type	10	10	7.3	2.43	33		203		.73
	15	6	3.53	1.24	35			100	.59
Inst.	15	10	9.13	3.08	34			259	.91
U-type	15	12	13.40	4.16	31			380	1.12
	15	15	11.67	2.49	21			331	.78
Alt.									
All.	15	4	2.33	.80	34	100			.58
	15	8	3.27	1.39	43	140			.41
W	10	8	2.00	.80	40				.25
S	10	8	3.20	1.68	53				.40

difficulty increases more rapidly than length. The figures in the 6th column express the difficulty of each maze in terms of percent of the mean trials of the 4-alley maze. The 7th and 8th columns express the same relationship in terms of the 5 and 6-alley lengths respectively. There is only one case of a drop in percentage and that is found in going from the 12 to the 15-alley instructed group. Even here, however, when compared with the 6-alley maze, the 15-alley length is more difficult per unit, since the percentage is 331 and it need be only 250 to be as difficult as the 6-alley maze.

The last column on the right, which is probably the most significant, gives the score or difficulty per cul-de-sac. This score is obtained by dividing the mean number of trials for each maze by the number of alleys in that maze, and indicates whether increase in difficulty is proportional to increase in length. If the difficulty increases more rapidly than the length, the figures in this column for any one type of maze should form an ascending progression from those for short to those for long mazes.

It was found that all of the X-type in the first section of the table fell into such a progression, and the U-type in the same section fell into a similar though not quite so steep a progression. The 2 X-type and the 4 U-type mazes with the fuller instructions once more gave similar progressions, but ones which were on the whole still less steep. The findings, then, in these four experimental situations are similar to those in the verbal field. They are, with two exceptions, particularly similar to the findings of Robinson and Heron.[17] That is, an increase in length not only increases the difficulty as a whole, but increases the difficulty per unit. These two exceptions are the 5-alley X-type group and the 15-alley U-type more fully instructed group. They are discussed later in the paper. Allison's two mazes, in keeping with his findings, also offer exceptions to this greater increase in difficulty with increase in length.

Warden's two mazes are of the same length so no comparison can be made between them. Their data, however, have been included in order that they might be compared in difficulty with the conventional mazes. It is evident that they, along with Allison's mazes, are much less difficult per alley. Their absolute difficulty, which can be obtained from the means column, is of course also much less than that of the conventional mazes.

The effect of the two degrees of instructions can be seen from the means column and from the last or 'score per cul-de-sac' column. In both cases, as might be expected, the scores of the more fully instructed groups are much lower than the scores of the other groups which learned conventional mazes. The same is also true in terms of errors and time.

The results of this study are given in terms of errors in Table III.

The second column in Table II, the one giving the number of S's, is omitted from Table III. Otherwise the error data are presented in the same order as the trial data. There are no exceptions whatever in this table to the fact that difficulty increases more rapidly than length. Even Allison's 8-alley

[17] Robinson and Heron, *op. cit.*

TABLE III

ERRORS

	No. Cul-de-sac	Mean	M.D.	V.	Percent 4 Cul-de-sac	Percent 5 Cul-de-sac	Percent 6 Cul-de-sac	Score per Cul-de-sac
X-type	4	5.80	3.33	57	100			1.45
	5	12.00	6.40	53	207	100		2.40
	6	17.93	7.29	41	309			2.99
	8	38.93	29.10	75	671			4.87
	10	62.20	38.32	62	1072	518		6.22
U-type	6	14.53	4.43	37			100	2.42
	10	37.98	19.26	51			261	3.80
	12	57.07	26.87	47			393	4.76
Inst. X-type	5	4.40	2.20	50		100		.88
	10	16.90	7.06	42		384		1.69
Inst. U-type	6	5.00	2.13	43			100	.83
	10	20.20	6.61	33			404	2.02
	12	28.67	10.76	38			573	2.39
	15	37.67	11.42	30			753	2.51
Alt. All.	4	2.13	1.36	64	100			.53
	8	4.33	2.80	65	203			.54
W	8	2.00	2.00	100				.25
S	8	3.80	2.76	73				.48

maze is slightly more difficult per unit than the 4-alley one, the scores per cul-de-sac being .54 and .53, respectively. Such a jump, however, from half size to full size, in the conventional mazes doubles or trebles the score per cul-de-sac rather than raises it merely .01.

The results in terms of time are found in Table IV.

The one exception in this table to the greater increase in difficulty in proportion to length is that of the 15-alley more fully instructed group.

DISCUSSION OF EXCEPTIONS

The exceptions referred to above in connection with Table II, the only two found in the 14 groups which learned conventional mazes, are the 5-alley X-type group and the 15-alley U-type more fully instructed group. The exception in the 5-alley group is only in terms of trials and not in terms of errors and time and can be dismissed as being of little importance. The fact that it is preceded by one shorter and fol-

TABLE IV

TIME

	No. Cul-de-sac	Mean	M.D.	V.	Percent 4 Cul-de-sac	Percent 5 Cul-de-sac	Percent 6 Cul-de-sac	Score per Cul-de-sac
X-type	4	120.13	60.53	50	100			30.00
	5	214.25	106.10	50	178	100		42.85
	6	349.80	188.70	54	291			58.33
	8	602.67	412.10	68	502			75.33
	10	948.70	525.80	55	790	443		94.87
U-type	6	306.27	134.40	44			100	51.04
	10	698.42	361.38	52			228	69.84
	12	892.80	453.00	51			293	74.40
Inst. X-type	5	78.00	34.80	45		100		15.60
	10	235.50	60.10	26		302		23.55
Inst. U-type	6	108.33	34.20	32			100	18.06
	10	445.67	154.60	35			411	44.57
	12	541.87	218.80	40			500	45.16
	15	612.80	187.53	31			566	40.85
Alt.								
All.	4	58.80	27.12	46	100			14.70
	8	112.67	53.87	48	192			14.08
W	8	81.20	36.00	44				10.20
S	8	122.70	47.70	39				15.34

lowed by 3 longer mazes which form an ascending progression is evidence that this group is simply an exception to the general rule in terms of trials. This exception is most probably due solely to the fact that there are 3 extreme S's in the group. The 15-alley U-type instructed group offers an exception, however, not only in trials but also in time. It does not occur in errors. Since this was the longest maze used. this exception cannot be so easily dismissed. It is our belief, however, that it is an *exception* and that if still longer mazes were used the difficulty per unit would again increase. Two possible causes are given for this exception. In the first place this and the 6-alley instructed group were the last groups to be finished and the S's were somewhat more highly selected than those of any other groups. They were the only groups which did not contain any first semester freshmen, and since most of the S's were sophomores who were taking a second course in psychology, it is possible that the records for both groups are better than they should be. Some of these S's

also had already had some maze experience, having learned another maze about a year previous to this experiment. In the second place, all four, or at least the first three lengths of the U-type mazes were made too easy by the fuller set of

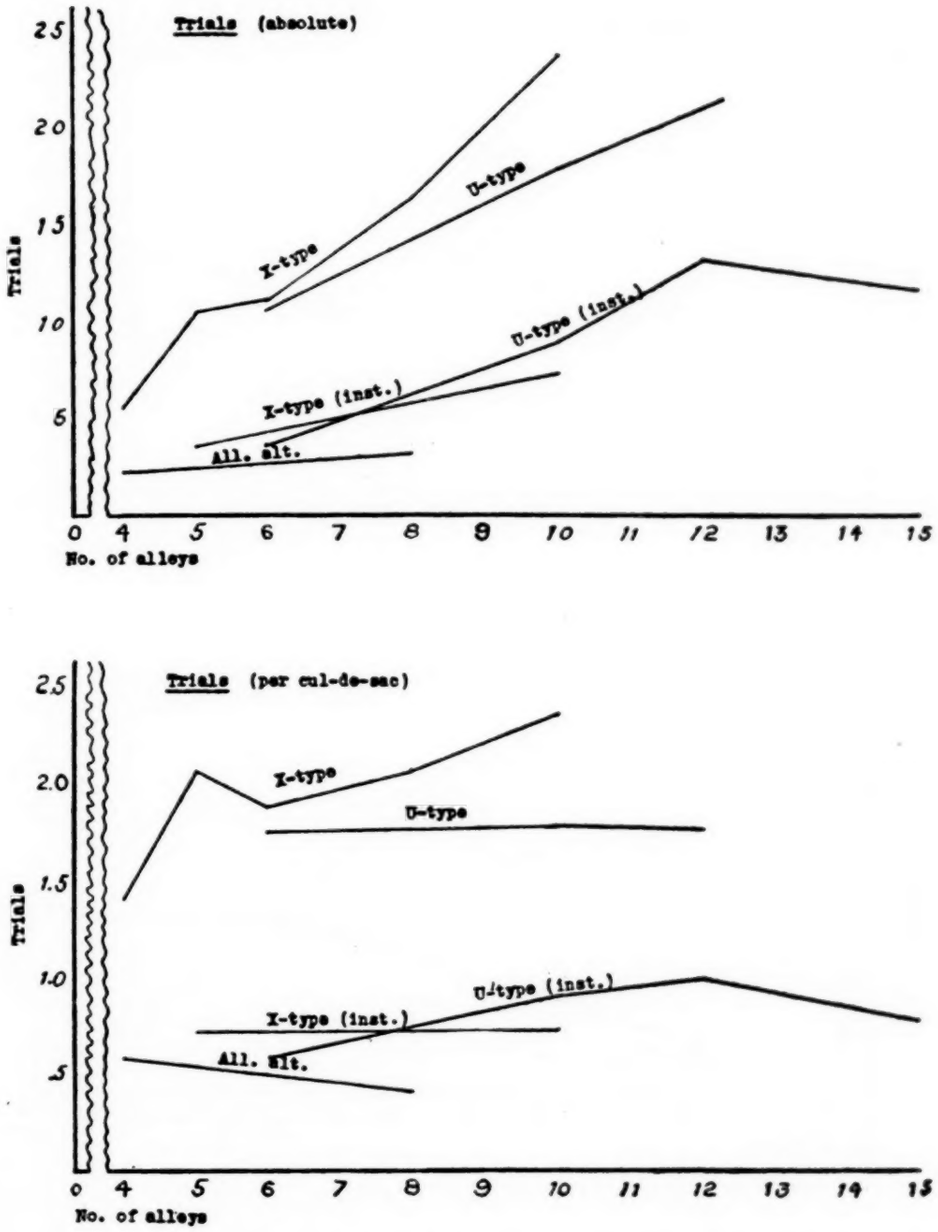

FIG. 1.

instructions. The shorter of these mazes appeared so easy on the first trial that there was a tendency on the part of the S's not to take the instructions seriously. That is, it didn't seem necessary to concentrate and try hard to solve the maze.

They knew that they could solve it without the extra exertion. This condition was less true for the longer and was of course least true for the 15-alley length. For this reason it is believed that the S's lost the least time in making a determined effort

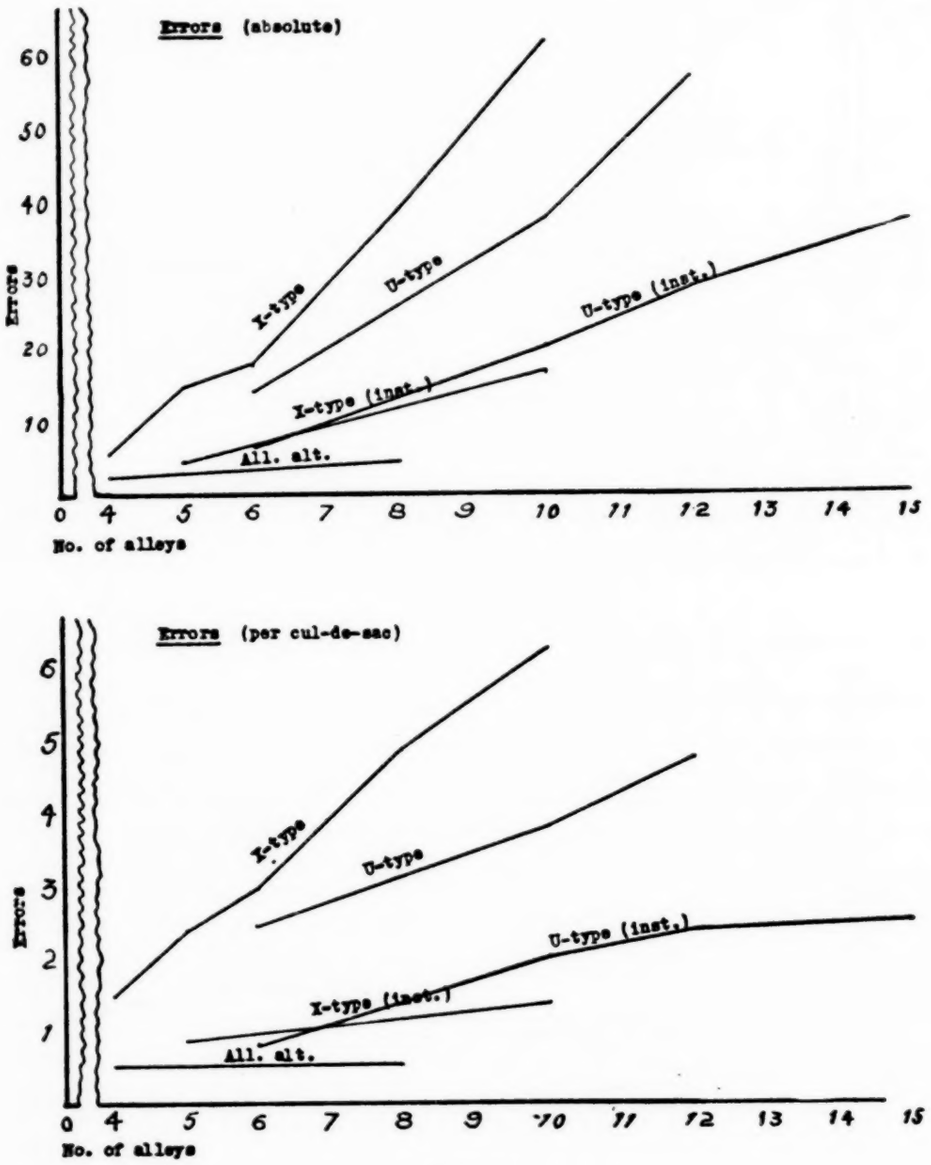

FIG. 2.

on this maze and consequently made a relatively better record than might otherwise be expected.

PROGRESSION CURVES

Figure 1 shows the progression curves for the trial data presented in Table II. The curves in the upper half of the

figure are drawn in terms of the mean number of trials. They show the increase in absolute difficulty accompanying increases in length. The curves in the lower half are drawn in terms of the number of trials necessary per cul-de-sac and indicate whether the difficulty per unit increases or decreases.

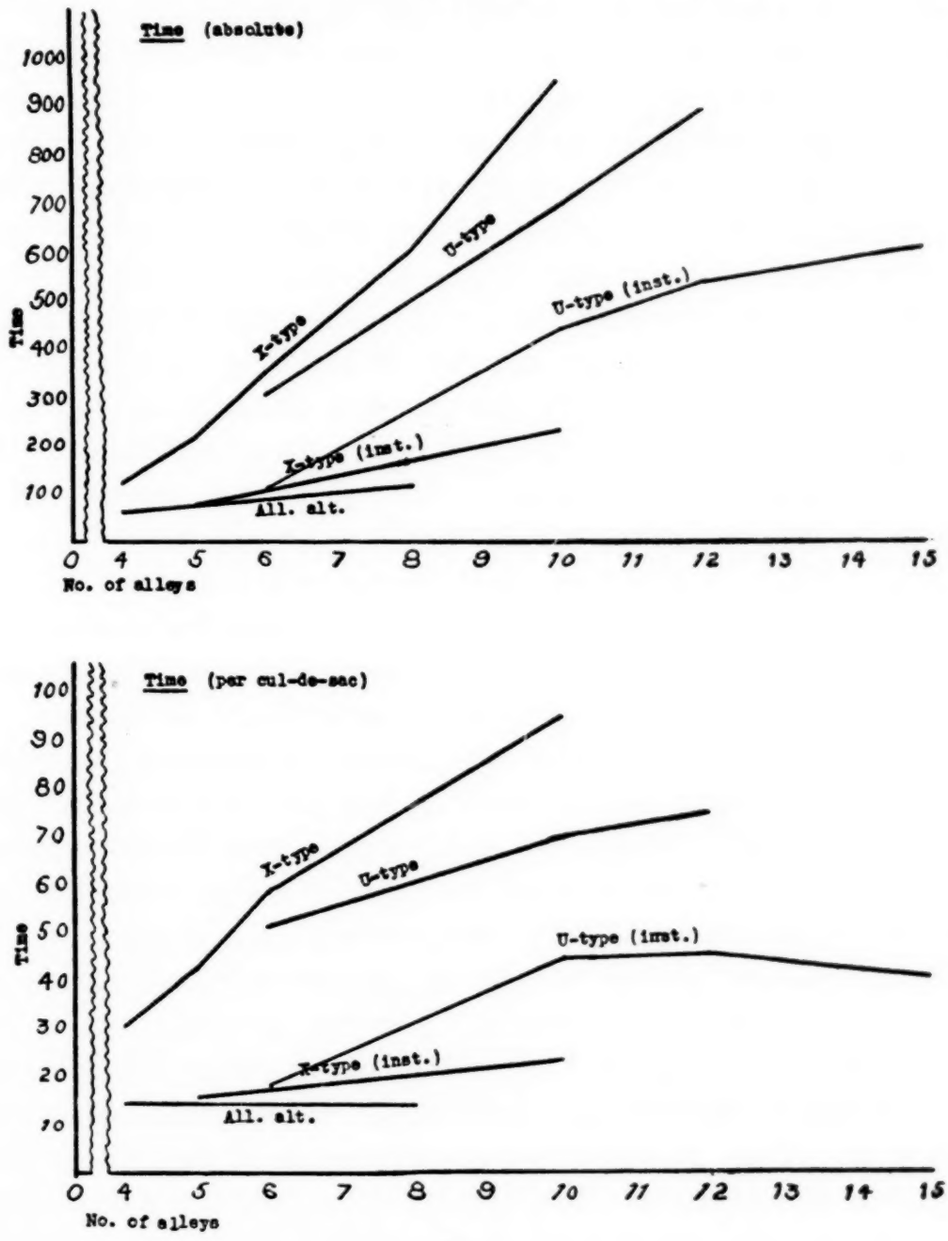

FIG. 3.

Figure 2 shows the progression curves for the error data presented in Table III and Fig. 3 for the time data in Table IV. These curves represent graphically the data presented in the 'mean' and the 'score per cul-de-sac' columns.

Comparison with Verbal Learning

Figure 4 gives a comparison between the maze results and Robinson and Heron's [18] verbal nonsense syllable results. The curves in the upper half of the figure are Robinson and Heron's curves and are drawn in terms of the total number of units presented before the material is mastered. Those in the lower half are the maze curves and are drawn in a similar manner. This comparison, then, made in terms of the total number of units presented before the material is mastered, gives a measure of the effort necessary for mastery. One presentation of one syllable and one presentation of one unit of the maze are assumed to be the two constant units of effort. The numbers on the axes of ordinates were obtained by multiplying the mean number of trials or repetitions for each length of material by the number of units in that particular length. For instance, 5.67 trials were required to learn the 4-alley X-type maze. The product of 4 and 5.67 is about 23, the point at which the X-type progression curve begins. This, as Robinson and Heron point out, is the only way in which to compare the effort necessary for mastery of material of different lengths, since two presentations are not equal unless the number of units presented are the same.

All of Robinson and Heron's curves are positively accelerated with the slight exception of the last part of the one for the third cycle. This exception, as Robinson and Heron suggest, may be due in some manner to practice effect from having learned cycles 1 and 2. The two curves obtained from the less fully instructed groups are quite similar to these nonsense syllable curves, both showing positive acceleration. The curve obtained from the U-type instructed group is positively accelerated in the first part and negatively accelerated in the last part. Two reasons have already been advanced for this negative acceleration. The first of these, the too great selection of subjects, could have been avoided and to that extent the negative acceleration might be called exceptional. The second, the fact that most of these mazes were made too easy by the fuller instructions, seems inherent in the conditions of

[18] Robinson and Heron, *op. cit.*

this particular part of the experiment and to that extent we are justified in saying only that the negative acceleration probably is temporary. In either case it would not be likely to continue indefinitely with longer mazes. There is no

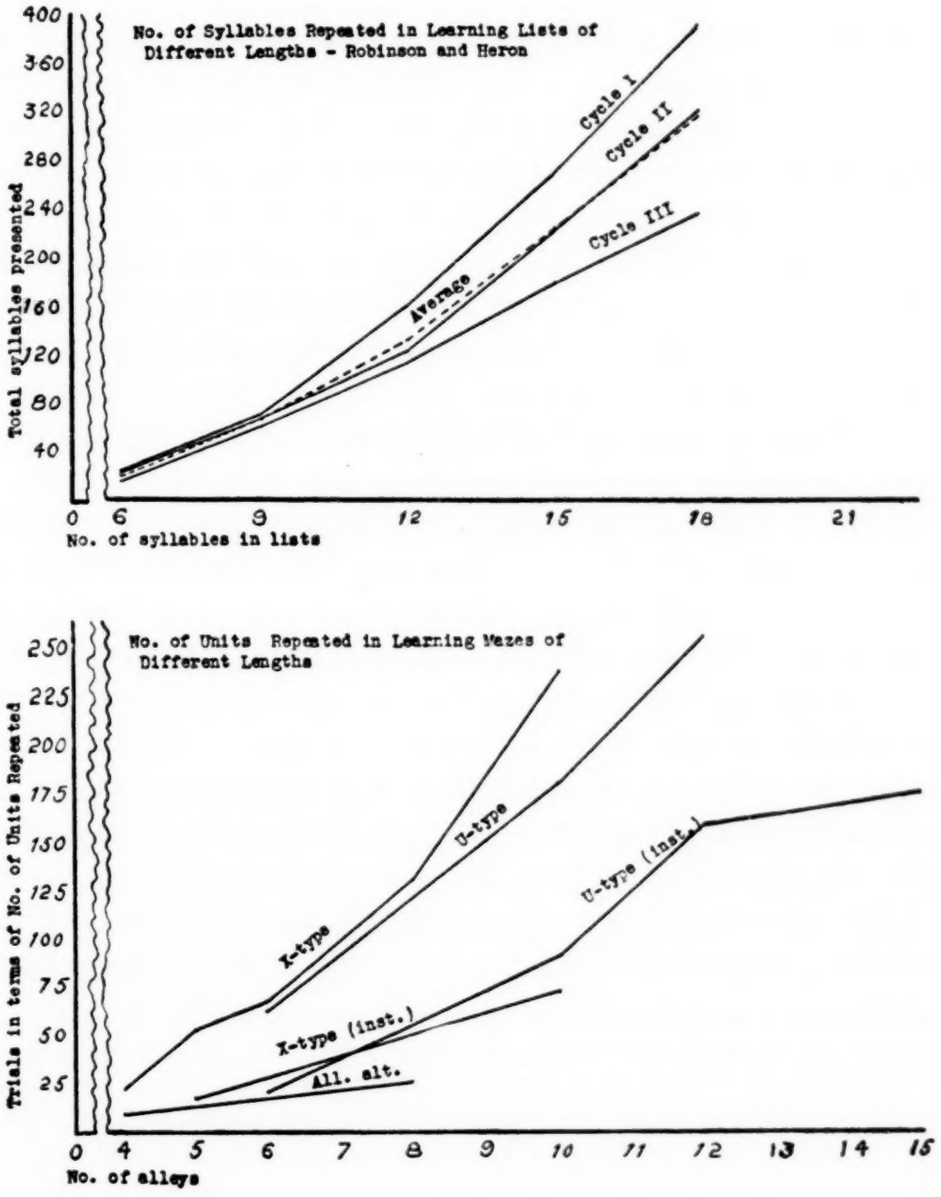

FIG. 4.

reason for holding the opposite view since that would imply, as Robinson and Heron have pointed out in the verbal field, "that after a certain point in length is reached, additional increases in length would not be accompanied by additional increases in difficulty." They continue, "It seems a sensible

expectation that in its later stages the time-length relationship will be linear or positively accelerated and that any negative accelerations which have appeared are temporary." [19]

Discussion and Conclusion

Several factors might be mentioned in explanation of the fact that it is relatively more difficult to learn a longer than a shorter list of nonsense syllables. The longer the series the greater the number of associations that must be formed between the members of the series; and the longer the series the greater the necessity, not only of holding together the separate members of the series, but of tying together groups or sections of these members in a unified whole. It seems evident also that the longer the list the less concentrated can be the attention upon any one association in any one repetition. Relatively more repetitions are necessary, then, to bring these associations to the level where they will function in a reproduction of the list. It is also true that the longer the list the greater will be the tendency for the later formed associations to confuse and be confused by the earlier formed ones. That is, retroactive and proactive inhibitions become increasingly evident with increase in length. All these considerations point to the necessity of a relatively greater expenditure of energy and an attendant greater degree of fatigue in the learning of a long list of nonsense material.

That practically the same factors have bearing on the relatively greater difficulty of a long task in serial motor learn-

[19] It should be pointed out here, however, that while the exception to the positive acceleration of Robinson and Heron's time-length curves (Fig. 4) is very slight, it is more marked in the curves which can be constructed from their data showing the relative difficulty of the lists of different lengths. The acceleration in these curves is greatest from 6 to 9 syllables and least from 15 to 18. McGeoch, J. A. and Obershelp, V. J., The influence of length of problem and of transfer upon rational learning and its retention, *J. Gen. Psychol.*, 1930, **4**, 154–170, have obtained data in rational learning from which the same type of curves could be constructed, there being a marked acceleration from 6 to 12 items and a much slighter acceleration from 12 to 18. Skaggs, E. B., The relation of length of material and number of repetitions needed to learn, *J. Gen. Psychol.*, 1929, **2**, 150–152, and others, have suggested the possibility that this falling off in degree of acceleration or leveling off of the curve of difficulty at a certain place is a characteristic of such curves, but that it is a temporary phenomenon and that these curves would become positively accelerated again if longer lists were used.

ing seems a justifiable conclusion. There is much similarity between verbal and motor learning of a serial nature. First, it may be said that no learning process in purely verbal and that relatively few on the human level at least, are purely motor in nature. The terms 'purely verbal' and 'purely motor' simply refer to the extreme points on the behavior scale with most human learning activity falling somewhere in between. The two processes under discussion are far from the extremes, and are what are commonly called combinations of verbal and motor reactions. In the learning of a list of nonsense syllables the procedure usually consists in pronouncing each syllable as it is presented and later anticipating its appearance by speaking it. The mastery of a list of nonsense syllables consists in building up a certain series of motor responses. Motor behavior (speech, implicit or explicit) is thus a part of the process of learning verbal material.

Verbalization, on the other hand, plays an important role in serial motor learning. Most S's verbalize during the learning process, and when they have learned the maze, have built up a series of verbal responses such as 2 right, 1 left, etc. Verbalizations of this kind when used to guide the finger over the wire or the stylus through the groove are not radically different from the repetition of nonsense syllables. The same phenomena that were mentioned in explaining the greater difficulty of long lists of nonsense syllables seem also to enter into the maze situations. Retroactive and proactive inhibitions are particularly evident.

Six groups of subjects, it will be recalled, were given fuller instructions in regard to the nature of the mazes and were requested to use what was considered to be the best method of attack, the verbal method. All subjects were asked to *memorize* and *group* moves in order that the maze situation might be made as similar as possible to the verbal nonsense situation. When a subject had learned a maze he had a series of numbers in mind which represented the sequence of moves and changes of direction in the maze. Otherwise the number series was as meaningless as a number series or nonsense list in a verbal experiment and might as well have

been learned in such a situation. Other reasons for giving these fuller instructions were as follows: (1) Not all subjects, as can be inferred from the above discussion, verbalize in maze learning. Of those who do, some verbalize more than others, and thus differences are introduced into the data which should not be there, since a rather high correlation exists between the degree of verbalization and the speed with which the maze is learned. Such differences in the maze records are due primarily to differences in methods of attack rather than to individual differences in learning ability, and should be prevented. The few S's who learn mazes in a motor manner do not seem to be aware of any verbalization. There is, however, probably some verbalization present below the level of awareness, else they would be even longer in learning the mazes. On the other hand, it is quite evident from their behavior that they do at least verbalize less than other S's. Their behavior, too, is different from that of S's learning nonsense syllables, and the fuller instructions were given to eliminate in large measure this difference, by making more homogeneous the methods of attack. (2) It is possible that the maze situation is a little more unfamiliar to the majority of subjects than the verbal learning situation, and is thus more susceptible to emotional interference. The fuller instructions familiarize them with the maze and the procedure of learning and do much toward removing this emotionality.

It will be remembered that these groups with the fuller instructions gave a progression less steep than the others, and possibly less steep than the curves for the memorization of nonsense material. Two reasons for this are given below: (1) As has already been pointed out, the fuller instructions were possibly too complicated for the shorter mazes. Some concentration and effort was necessary for the carrying out of these instructions, and the subjects found out that the shorter mazes did not demand this expenditure of energy. Such an expenditure, however, did become increasingly necessary with the longer, and was, of course, most necessary with the 15-alley maze. The greater the concentration and effort, the sooner the maze was learned. This, of course, would tend to

flatten the curve. (2) If these instructions reduced the amount of emotionality and random useless behavior in the learning process, which they apparently did,[20] the effect (in absolute sense) would, of course, be greatest in the longer mazes, and this might be another factor accounting for the less steep progression in the curves for these groups. This also leads one to speculate in regard to the probable effect of specific instructions as to the method of learning nonsense syllables. Such instructions would be of great help to the novice who had no method of attack and was making no attempt to form associations. Such a subject is comparable in a way to the 'motor' maze subject, and with him eliminated by means of instructions, the effect might be the same in verbal learning as that which was evident in serial motor learning. In fact, this is about what happens in the third cycle of Robinson and Heron's results. The instructions were the same as those for the other two cycles but the subjects were more experienced. They had already learned two cycles and evidently had improved their methods of attack. Experience, then, which it seems would be the equivalent of fuller instructions, reduces the positive acceleration of this curve, and in fact, causes the last part of it to be slightly negatively accelerated.

Whether or not the foregoing explanations and assumptions are valid, there remains objective evidence of the similarity between the two processes of learning.

Summary

1. We find that the Warner-Warden maze is not a suitable instrument for testing the relationship existing between length and difficulty of material in motor learning. This is probably true for rats as well as for human beings.

2. If a certain amount of caution is exercised in avoiding patterns which are obviously very easy, we find that the X- and U-types of conventional mazes are suitable for testing this relationship.

[20] The variability of the groups learning mazes under fuller instructions was considerably less than that of the other groups, as can be seen by comparing the coefficients of variability for these groups in Tables II, III and IV.

3. In the motor field it would seem that as material becomes longer, it becomes relatively more difficult to master.

4. When this relationship is compared to that found to exist in verbal learning, a marked similarity is evident.

(Manuscript received July 18, 1932)

STUDIES IN CROSS EDUCATION. II. FURTHER EXPERIMENTS IN MIRROR TRACING THE STAR-SHAPED MAZE

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Acadia University

EXPERIMENT *A*

Transfer from One Hundred Trials Practice

In an investigation reported in a previous number of this journal,[1] it was found that 100 trials practice in mirror tracing a star-shaped maze resulted in a large amount of transfer from the left foot to the right foot, left hand, and right hand (Groups B and C, 20 Ss), and from right hand to left hand, right foot, and left foot (Group A, 10 Ss). With these subjects it was found impossible to take more than ten transfer trials.

In the present experiment, therefore, a fourth group of 10 subjects (Group D) was given 100 practice trials in tracing the maze with the right hand, followed by three 'transfer' trials with the left hand, three trials with the right foot, and 100 trials with the left foot. In all respects except the order and number of transfer trials, the procedure with Group D was the same as that used with Group A.[2] As before, time was the measure, timing was done with a stop-watch, and all records (as elsewhere in this paper) are in seconds.

Table I shows that as far as the first three transfer trials with the left hand, right foot, and left foot are concerned, the amount of transfer for Group D differs little from a similar measure of the transfer in the first three groups. The average time of the first three transfer trials of Group D is: left hand, 8.1 seconds; right foot, 11.7 seconds; left foot, 12.2 seconds. For Group A, which also received 100 trials practice with the

[1] Cook, T. W. Studies in cross-education, I. Mirror tracing the star-shaped maze, *J. Exper. Psychol.*, 1933, 16, 144–160.

[2] *Ibid.*, page 149.

TABLE I

FIRST THREE PRACTICE AND TRANSFER TRIALS, GROUP D

Trial	Practice Series Right Hand	Transfer Series		
		Left Hand	Right Foot	Left Foot
1	35.0	8.4	13.7	14.5
2	24.8	7.6	10.5	10.9
3	19.7	8.3	10.9	10.2
Group D	Average of first three trials, 26.5	8.1	11.7	12.2
Group A	Average of first three trials, 26.5	7.4	10.7	14.3

right hand, the time for the corresponding transfer trials was: left hand, 7.4 seconds; right foot, 10.7 seconds; left foot, 14.3 seconds. The average number of seconds taken in the first three practice trials with the right hand was 26.5 seconds for both Group D and Group A. In comparison with the amount of transfer, the differences between the first three transfer trials of Group A and Group D are relatively small. Further discussion of group differences in amount of transfer will be deferred, since the question is complicated by two factors, the order of transfer trials, and differential transfer to the various muscle groups, both of which are discussed in a later section. If a more detailed comparison is desired at this point, Table I of the present paper and Table IV of the previous report give averages of practice and transfer trials for all four groups.

The results of 100 transfer trials of Group D with the left foot appear in Fig. 1, which should be studied in conjunction with Fig. 4 of the previous article.[3] In Fig. 1 the solid lines represent 100 trials with the left foot, ten trials a day. The group averages for each of the first ten trials are plotted individually. Later points are averages of each day's trials. The upper solid line in Fig. 1 shows the average of the practice trials for Groups B and C, the lower solid line the transfer trials for Group D.

Comparison of the transfer curve for Group D with the practice curve for Groups B and C should give some information about the permanence of the transfer. Does the influence

[3] *Ibid.*, page 151.

of previous practice wear off in the course of the first few transfer trials, or does it continue through the whole transfer series? If Group D is equal in ability to the average of Groups B and C, we may accept our results as evidence for the latter hypothesis. The transfer curve for Group D is lower

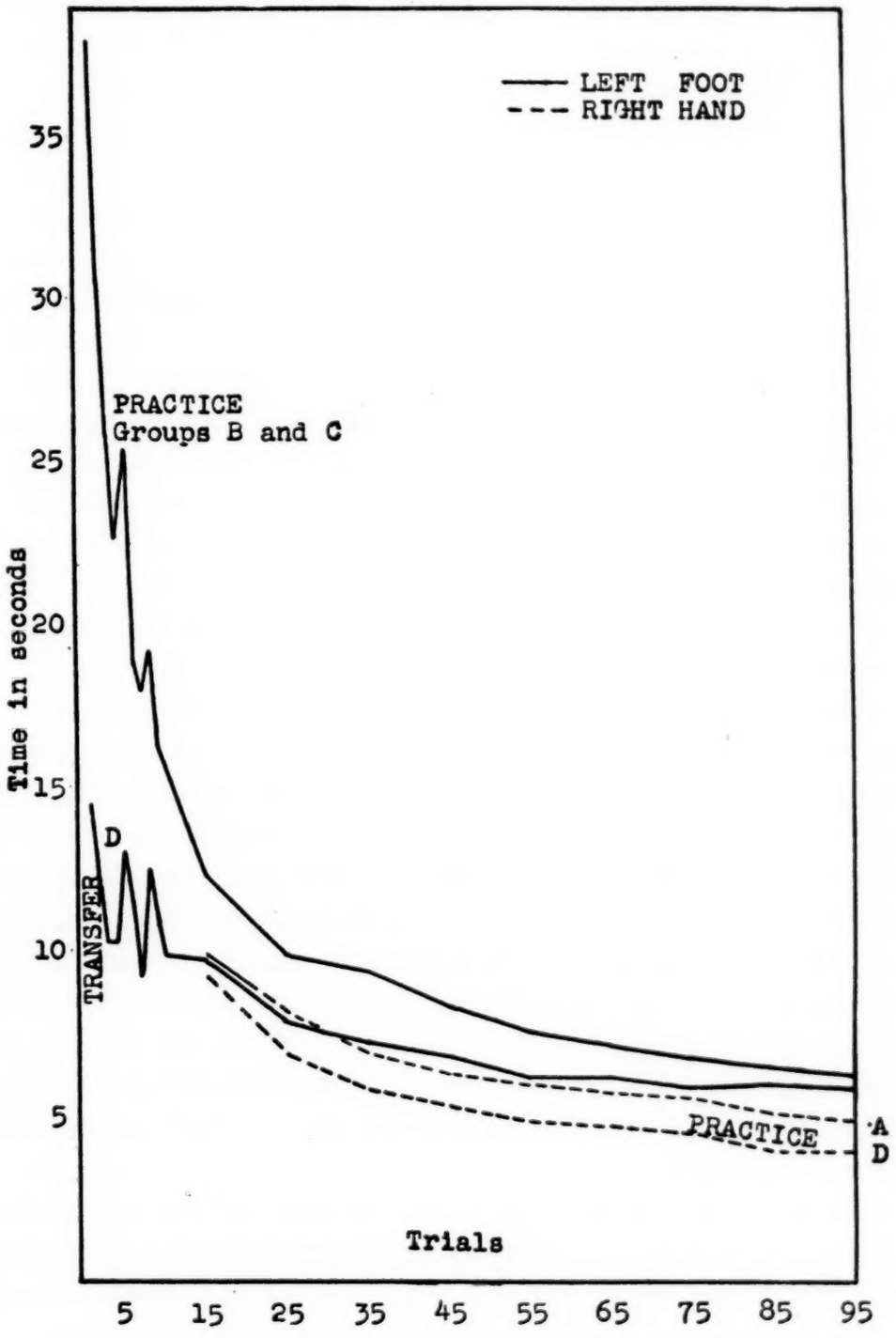

FIG. 1. Comparison of 100 practice and transfer trials with the left foot.

than the practice curve for Groups B and C throughout. There are two ways in which we may arrive at an estimate of the comparative ability of the different groups: (*a*) Comparison of Groups B and C with the left foot, or Groups A and D with the right hand, in the practice trials in mirror tracing. By this criterion, Fig. 4 in the report of our previous experiment shows Groups B and C very nearly equal, Group C being slightly faster than Group B in the last thirty trials. For convenience in comparison the last 90 practice trials of Groups A and D are shown by broken lines in the lower part of Fig. 1.[4] Here Group D is seen to be about one second faster than Group A. (*b*) The only direct information concerning the relative ability of all four groups comes from the preliminary practice trials without the mirror. In these orientation trials the right-left differences were very small. The right and left foot, however, were fifty percent slower than the right and left hand. Before averaging hand and foot records for each group, I accordingly weighted the time records for the hands fifty percent. The resulting averages of right and left hand, right and left foot records are: Group A, 6.95 seconds, Group B, 6.65 seconds, Group C, 6.21 seconds, Group D, 6.28 seconds. In speed of tracing with direct view of the maze, Groups A and D differ by 6.95 minus 6.28, or .67 second, a little less than the difference between their practice curves in mirror tracing shown in Fig. 1. The difference is, however, in the same direction. Group A is slower than either Group B or Group C, while Group D is about .15 second faster than the average of Groups B and C. If we subtract this .15 second difference from the .38 second difference between the last point of the left foot practice (Groups B and C) and transfer series (Group D), as indicated by the solid lines in Fig. 1, we have a remainder of .23 second which could be attributed to the effect of the preceding 100 practice trials on the last 10 transfer trials of Group D.

Theoretically, since the effect of the mirror reversal is overcome to a large extent after ninety trials in mirror tracing, the orientation trials with direct view of the maze should

[4] The first ten practice trials of Groups A and D were practically identical.

give a fair index of the relative ability of the groups in the last few trials in mirror tracing. Unfortunately for a-priori deductions, the correlations between individual records on the two measures are low. By the method of rank differences, the correlation between individual scores on the last 10 practise trials in mirror tracing and scores for the same individuals in the orientation trials is .52 for the twenty subjects of Groups A and D, and .22 for Groups B and C. These correlations, though positive, are too small to allow us to attach much weight to group ranking in the orientation trials as a means of predicting group standing in the last 10 practice trials in mirror tracing. It does, however, seem reasonably certain that Group D is not more than one second (the difference between Groups A and D in the practice trials, dotted lines, Fig. 1) faster than the average of Groups B and C. Since the transfer curve for Group D in Fig. 1 is more than one second lower than the practice curve for Groups B and C during the first fifty trials, our results may be taken as evidence that the effect from 100 practice trials with the right hand persists for fifty transfer trials with the opposite and unsymmetrical left foot.

Amount of Transfer to Different Muscle Groups

Data from the first three transfer trials of Group D may be combined with similar results from Groups A, B, and C[1] to find the amount of transfer to different muscle groups. From the records of the transfer trials of Group D in Table I we see that the right foot, on the same side of the body as the practiced right hand, makes a better record (11.7 seconds) than the left foot (12.2 seconds), which is opposite and unsymmetrical to the right hand. The advantage of the former, however, is much smaller than that of the right foot over left foot in the transfer trials of Group A, or the superiority of left hand (same side as left foot) over right hand (opposite and unsymmetrical to left foot) in the transfer trials of Groups B and C.[5] In all of the three last-named groups there is a markedly greater transfer effect to the hand or foot on the

[5] *Ibid.*, Table IV, page 153.

same side, than to the hand or foot opposite and unsymmetrical to the practiced left foot or right hand. Is the smallness of the superiority of right foot over left foot in the transfer trials of Group D only a chance variation, or is there a factor in our procedure that may account for it? The only obvious possibility is the order of transfer trials. This order for the different groups was:

	Practice Trials	Transfer Trials		
	1st.	2d.	3d.	4th.
Group B	Left foot	Right hand	Left hand	Right foot
Group C	Left foot	Right foot	Left hand	Right hand
Group A	Right hand	Left foot	Right foot	Left hand
Group D	Right hand	Left hand	Right foot	Left foot

The order of transfer trials for Group B is thus the reverse of the order for Group C, and for Group A the order is the reverse of that for Group D. With Groups B and C this reversal had no appreciable result. The average of the first three transfer trials for the hands which come first in order, the right hand with Group B and the left hand with Group C, is 9.2 seconds, while the average for the hands which succeed these in the transfer tests, the left hand with Group B and the right hand with Group C, is 9.0 seconds. On the contrary, the corresponding average of left foot (Group A) and right foot (Group D) is 13.0 seconds, as contrasted with an average of 11.5 seconds for right foot (Group A) and left foot (Group D). Even after 100 trials with the right hand, there seems to be some transfer from three trials with one foot to the foot which is tested later.

In any case, combining the records of the transfer trials of the right hand, Group B, with those of the right hand, Group C, left hand with left hand, and right foot with right foot, and similarly averaging the two left foot, right foot, and left hand records of Groups A and D, should equalize any such secondary transfer effects. To balance the influence of 'leftness' and 'rightness,' and superiority of hand over foot, and leave only the relation of the different muscle groups to

the right hand or left foot having the initial 100 trials practice, we average the results of the transfer trials of all four groups as described in detail in the previous report and outlined below.

The averages of the first three transfer trials of all four groups are shown in Fig. 2. The lowest, or 'Z' curve, repre-

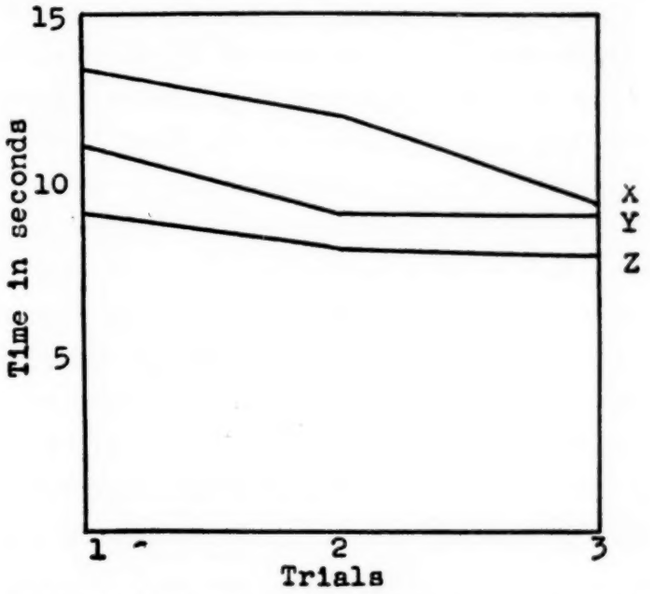

FIG. 2. First three transfer trials after 100 trials practice with left foot and right hand. Averages of Groups A, B, C, D. *X*, opposite and unsymmetrical; *Y*, same side; *Z*, opposite and symmetrical to practiced hand and foot.

sents the average time of the opposite and symmetrical muscle group (right foot, Groups B and C; left hand, Groups A and D), the second or '*Y*' curve the time of the muscle group on the same side (left hand, Groups B and C; right foot, Groups A and D), and the highest or '*X*' curve the muscle group opposite and unsymmetrical (right hand, Groups B and C; left foot, Groups A and D) to the practiced left foot and right hand.[6] These relationships are the same as those given for Groups A and C in Fig. 5 of the former report. The

[6] The method of comparison of opposite and unsymmetrical with the other two muscle groups involves the assumption that the average ability of the left foot and right hand equals the average ability of right foot and left hand. This assumption is supported by the results of 130 subjects in the 40 orientation trials without the mirror. The average of 2600 trials for right hand and left foot is 5.35 seconds, for left hand and right foot, 5.28 seconds—a difference of 1.4 percent. We have no means of making a like comparison for trials in mirror tracing, but there is reason to believe that the results would be similar. The hand-foot differences in speed are about the same in 'mirrorless' and mirror tracing trials.

greatest amount of transfer from hand or foot practice is to the opposite and symmetrical (*Z*), next to the same side (*Y*), and least to the hand or foot opposite and unsymmetrical (*X*) to the practiced hand or foot. As in our previous experiment, we may compute amount of transfer in percentage of the total direct gain from practice saved on the first transfer trial. The direct gain from practice is measured by subtracting the average time of the last ten practice trials for all four groups, 5.3 seconds, from the average time of the first practice trial, 36.6 seconds. The direct gain from practice is thus 31.3 seconds. The gain of the opposite and symmetrical muscle group on the first transfer trial is 36.6 seconds (time of the first practice trial) minus 9.15 seconds (time of the first transfer trial, *Z*, Fig. 2), or 27.45 seconds. In percentage terms this gain is 27.45 divided by 31.3, or 88 percent. A similar computation shows the transfer to the hand or foot on the same side, and the hand or foot opposite and unsymmetrical to the practiced left foot and right hand, to be 81 percent and 74 percent respectively.

Experiment *B*

Transfer from Ten Trials Practice

The experiment described in the present section represents an attempt to answer two questions: (1) Is the amount of transfer a function of the number of practice trials, or does some one phase of the learning occasion all the transfer? Bray, for example, concludes that "the slight improvement characteristic of hand practice after the first ten trials is not transferred." [7] (2) Does the transfer (if any) from the first few practice trials take place equally to all muscle groups, or do the relationships shown in Fig. 2 hold for small amounts of practice?

Four new groups (E, F, G, and H) of ten subjects each were selected. As before all were male students of Acadia University and Horton Academy. All the subjects used in the experiments reported in this paper were right handed. The four groups were given one session's preliminary practice of ten trials with right and left hand and foot with direct view

[7] Bray, Charles W. Transfer of learning, *J. Exper. Psychol.*, 1928, 11, 452.

of the maze. With Group E and Group F the order of these "mirrorless" trials was right hand, left hand, right foot, left foot, with Group G and Group H the order of trials was reversed. The trials in mirror tracing, which were taken on the day following the orientation trials, consisted of ten practice trials with the left foot or right hand, followed by ten trials with each of the other three limbs in the order indicated in Table II, reading from left to right.

TABLE II

ORDER OF PRACTICE AND TRANSFER TRIALS

	1st. 10 Trials Practice	2d. 10 Trials '10 Transfer'	3d. 10 Trials '20 Transfer'	4th. 10 Trials '30 Transfer'
Group				
E	Left foot	Right hand	Left hand	Right foot
F	Left foot	Right foot	Left hand	Right hand
G	Right hand	Left foot	Right foot	Left hand
H	Right hand	Left hand	Right foot	Left foot

The order of practice and transfer trials corresponded group for group in the present and previous experiment. One each of the groups having 100 and 10 practice trials used the same order of practice and transfer trials, *i.e.*, Groups B and E, C and F, A and G, D and H. The two experiments differed in the number of practice and transfer trials. For Groups A, B, C, and D the procedure was: practice trials (100), transfer trials (3), (3), (10 or 100). With Groups E, F, G, and H the trials were in the order: practice (10), transfer (10), (10), (10). The subjects in all groups were given the same instructions.

With Group D evidence was found that, even after 100 trials practice, three transfer trials with the right foot slightly influenced later trials with the left foot. It seemed probable that with 10 practice trials instead of 100, and 10 transfer trials instead of three, there would be a marked 'carry over' from transfer trials with one hand or foot to later trials with any other foot or hand. That such is the case is evident from Fig. 3. In Fig. 3 the solid lines indicate results for Groups E, F, G, and H. The 'practice' solid line (nearest the top)

in Fig. 3 represents the average of ten practice trials for the four groups: Groups E and F, left foot, Groups G and H, right hand. The solid line marked '10' in Fig. 3, is the average of

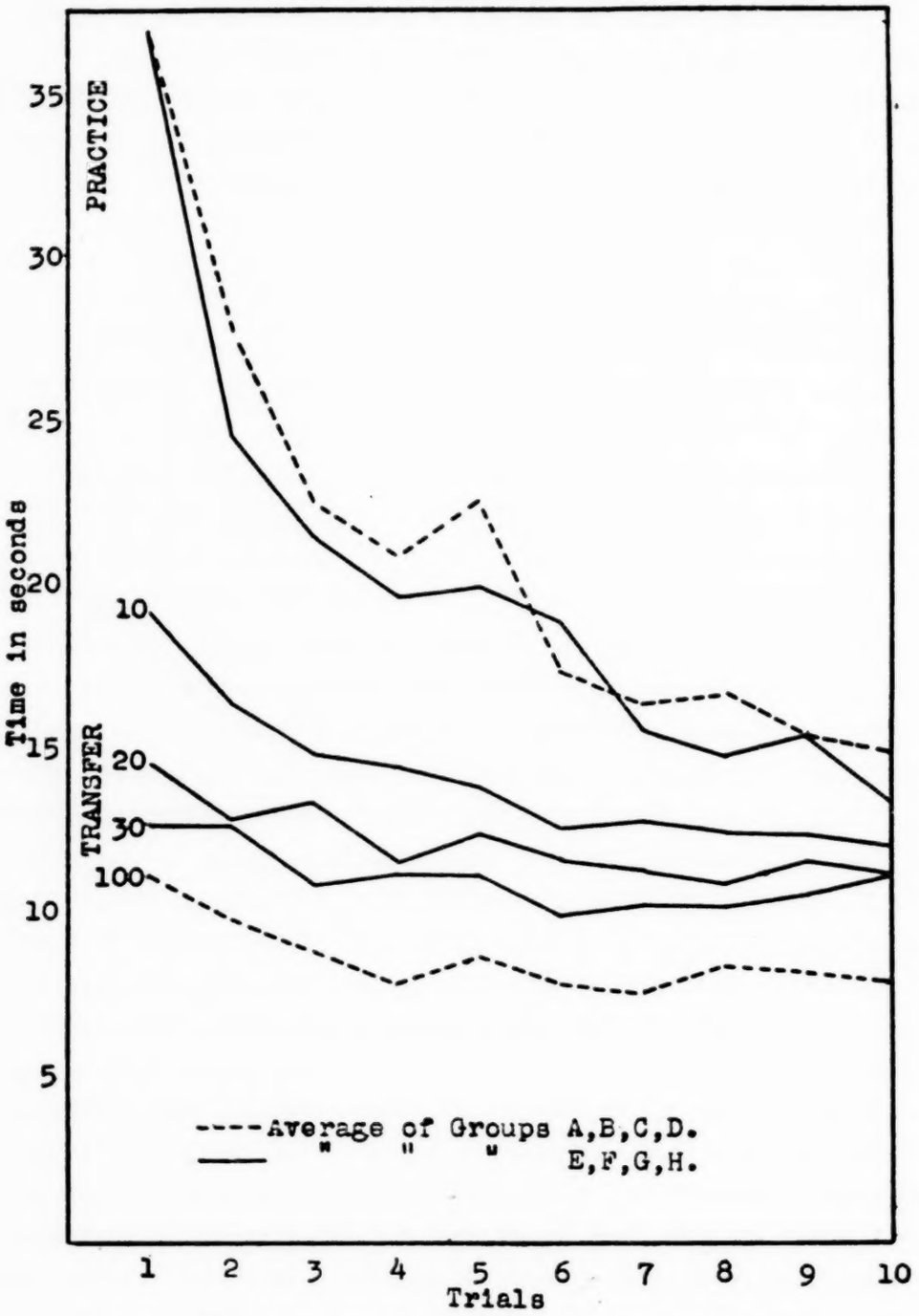

FIG. 3. Effect of number of trials practice on amount of transfer.

the right hand (Group E), right foot (Group F), left foot (Group G), and left hand (Group H). The trials with these four, which are also noted in the '10 transfer' column of Table

II, were preceded by ten practice trials. The solid line marked '20' in Fig. 3 represents averages of: Groups E and F, left hand, Groups G and H, right foot, as indicated in the '20 transfer' column of Table II. The trials taken by each member of this quartette were preceded by ten practice and ten transfer trials. Similarly the lowest solid line in Fig. 3 is the average of results from the right foot (Group E), right hand (Group F), left hand (Group G), and left foot (Group H). As appears in the '30 transfer' column of Table II, trials with each of the last-named four were preceded by ten practice and twenty transfer trials.

The upper of the dotted lines in Fig. 3 indicates the average of Groups A, B, C, and D in the first ten practice trials. In every way except possible group differences, it is entirely comparable with the solid line representing ten practice trials of Groups E, F, G, and H. Both are averages of the first ten practice trials with the left foot (Groups B, C, E, F) and right hand (Groups A, D, G, H). The lower dotted line shows ten transfer trials of Groups A, B, C, and D, following 100 practice trials with the right hand or left foot. The first three points on the latter curve are averages of all the first three transfer trials (each group used two hands and one foot or two feet and one hand) in the groups just named. With each of these four groups, however, only one hand or foot was given ten transfer trials. Consequently, the last seven points of the lower dotted line in Fig. 3 are averages of trials with the left hand (Group A), right foot (Group B), right hand (Group C), and left foot (Group D). The data for the '10' and '100' transfer curves in Fig. 3 were thus taken under conditions which differed in no significant respect except the number of preceding practice trials.

The '20' and '30' transfer curves in Fig. 3 are not strictly comparable with any of the other transfer curves. All data plotted in Fig. 3 represent an equal number of both right and left and hand and foot trials, and are therefore equated for right-left and hand-foot differences. But while the difference in procedure used with the '10' and '100' transfer groups is only a matter of the number of practice trials, and although

the '20' and '30' trials were prefaced by the same ten practice trials as the '10,' the additional ten or twenty trials were not taken with the hand or foot that began the practice. This difference in procedure for the '10' and '100' transfer data on the one hand, and the '20' and '30' on the other, may be appreciated best from a study of the successive columns of Table II.

It is apparent from a glance at Fig. 3 that the transfer is a function of both the first thirty and the last seventy practice trials. Also, it would seem that the amount of transfer is at least roughly proportional to the amount learned. The practice curve falls two-thirds as much with 10 trials practice as with 100 trials practice, and the drop on the first transfer trial after 10 trials practice is also two-thirds of a similar measure of transfer after 100 trials practice. The other transfer curves, though not wholly comparable with those following 10 and 100 trials practice, show a somewhat similar relationship between amount transferred and amount learned in the corresponding number of practice trials. More is learned from the tenth to twentieth than from the twentieth to thirtieth practice trials, and this is reflected in a greater difference between the '10' and '20' transfer curves than between the '20' and '30' transfer curves. In fact, if we plot the time of the first transfer trial after 10, 20, 30, and 100 practice trials, against the number of preceding trials practice, we get a typical negatively accelerated curve resembling closely the learning curve in Fig. 1.

The second question raised at the beginning of the present section, *i.e.*, whether the relative amount of transfer to different muscle groups is the same after 10 and 100 trials practice, can be decided only by comparison between groups, since it is plainly indicated in Fig. 3 that each successive ten transfer trials affects later trials with any other hand or foot. I therefore added five subjects to each of the four groups: E, F, G, and H, and obtained two new groups of fifteen subjects each. These fifty additional subjects took the same preliminary trials without the mirror as the previous subjects of Groups E, F, G, and H, but on the following day were given twenty

instead of forty trials in mirror tracing. These twenty trials were distributed as follows:

Group	1st. Ten Practice Trials	2d. Ten Transfer Trials
E	Left foot	Right hand
F	Left foot	Right foot
J	Left foot	Left hand
G	Right hand	Left foot
H	Right hand	Left hand
I	Right hand	Right foot

When we combine the results from the fifty additional subjects with the records for the first ten practice and ten transfer trials already taken with forty subjects of Groups E, F, G, and H, we have three groups of fifteen subjects each (Groups E, F, J) having ten trials practice with the left foot followed by ten transfer trials with one of the other three limbs, and three other groups (Groups G, H, I), also of fifteen subjects each, practicing ten trials with the right hand succeeded by ten trials with the left hand, or right foot, or left foot. The averages of the time records in practice and transfer trials of the six groups appear in Table III.

Figure 4 gives some data (in addition to the comparison already made from Fig. 3) on the amount of transfer from 10 as contrasted with 100 trials practice. All three solid lines in the left part of Fig. 4 are plotted from trials taken with the right hand. The upper curve represents the average of the first ten practice trials with the right hand for sixty-five subjects (Groups A, D, G, H, I). The curve marked 'E' is taken from the transfer trials of Group E (15 Ss) with the right hand, as given in Table III. The lowest solid curve, marked 'C,' represents ten transfer trials of Group C after 100 practice trials. All transfer trials shown in the left graph were preceded by practice with the left foot. The difference between the 'C' and 'E' curves gives the differential effect of 100 and 10 practice trials. A similar comparison for transfer trials with the left foot following right hand practice may be made from the solid lines in the right half of Fig. 4. Here the practice curve is the average for sixty-five subjects (Groups B, C, E, F, J). The solid line labeled 'G' is derived from the

TABLE III

AVERAGES OF TEN PRACTICE AND TEN TRANSFER TRIALS. FIFTEEN SUBJECTS IN EACH GROUP. TIME IN SECONDS

Practice Trials

Trial	Group H	Group I	Group G	Group F	Group J	Group E
	Right Hand			Left Foot		
1	34.1	32.4	32.0	37.2	34.4	37.8
2	21.8	27.4	20.7	29.2	27.1	27.1
3	17.6	17.7	18.6	24.4	24.5	22.7
4	17.1	15.1	17.1	21.2	21.7	18.1
5	15.4	14.2	13.3	20.7	17.9	16.8
6	15.3	14.3	13.1	21.4	19.4	20.0
7	14.1	12.9	11.6	17.4	17.2	16.9
8	13.6	13.2	10.8	16.9	14.7	15.9
9	12.4	10.8	11.6	18.3	14.3	16.7
10	11.7	11.2	11.2	13.1	14.7	12.7

Transfer Trials

	L.H.	R.F.	L.F.	R.F.	L.H.	R.H.
1	18.9	22.6	21.0	16.6	17.3	24.5
2	13.7	19.2	18.3	15.1	14.0	16.4
3	13.2	18.3	17.2	14.0	12.6	16.4
4	11.7	18.4	15.7	12.9	12.3	15.0
5	12.3	14.7	16.3	12.6	11.8	12.5
6	10.2	15.0	15.9	11.7	10.7	11.9
7	10.7	14.2	15.8	11.8	10.2	12.0
8	11.0	15.9	17.0	10.7	9.7	11.1
9	9.6	14.4	13.6	11.9	9.5	11.8
10	9.8	13.7	15.7	10.3	9.3	10.3

results of ten transfer trials of Group G (15 Ss) after ten trials practice, and 'D' shows results of a like number of transfer trials of Group D after 100 trials practice. We see that, as far as the amount of transfer from 10 and 100 trials practice is concerned, the relationship shown in Fig. 3 holds roughly true for Fig. 4: the amount transferred is proportional to the amount learned. The comparison in Fig. 3 is more reliable, however, since all curves are derived from the records of the forty subjects as contrasted with fifteen and ten subjects represented in the transfer curves of Fig. 4.

Transfer from Hand to Foot Compared with Transfer from Foot to Hand

The left and right graphs in Fig. 4 also permit comparison of the amount of transfer from hand to foot and foot to hand.

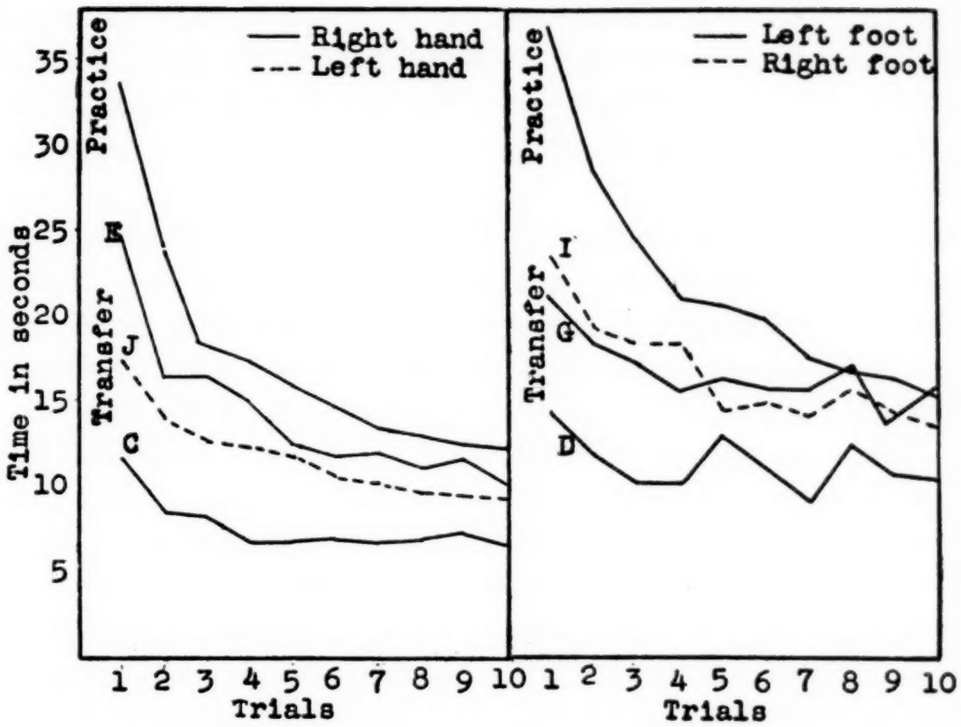

FIG. 4. Comparison of transfer from hand to foot and foot to hand. Upper curve on each graph practice trials for 65Ss. Other curves plotted from transfer trials of group indicated on the left of each curve.

The transfer curves in the left graph are from trials with the right hand (solid lines) and left hand (dotted line) after left foot practice. The corresponding curves in the right graph represent transfer trials with the left foot (solid lines) or right foot (dotted lines) after right hand practice. The left graph thus shows the transfer from foot to hand, and the right graph the transfer from hand to foot. As far as our data go, there seems to be little difference between hand-foot and foot-hand transfer. The 'C' and 'D' transfer curves, taken after 100 trials practice, are about the same distance below their respective practice curves. After ten trials practice with the left foot, there appears to be a little less transfer to the right hand ('E' curve) than from an equal amount of practice with the right hand to the left foot ('G' curve). However, the reverse holds for transfer from left foot to left hand ('J' curve) as contrasted with transfer from right hand to right foot ('I' curve). Group variation in ability is the most probable explanation of these discrepancies. If we average the transfer

from left foot to left and right hand, and compare the result with the average transfer from right hand to right and left foot, we find little difference in amount of transfer from foot to hand and hand to foot.

Amount of Transfer to Different Muscle Groups

As a first step in determining the presence or absence of a difference in amount of transfer to opposite and symmetrical, same side, or opposite and unsymmetrical muscle groups, after ten trials right hand or left foot practice, it was necessary to equate the groups as carefully as possible. This equating could be done for either the orientation trials without the mirror, or the first ten practice trials with the mirror. Since our transfer trials were in mirror tracing, the latter seemed the better method. It could not be used, of course, in determining the major division into forty-five subjects having initial practice with the right hand, and forty-five subjects having practice trials with the left foot. That division was made at random. The number of subjects in the two major groups, however, was probably large enough to secure approximate equality by operation of chance. In any case, a small difference between initially practiced right hand and left foot groups is of no importance for our present problem of transfer to different muscle groups, since the results for each of the three muscle groups (opposite and symmetrical, etc.) included one group having practice trials with the right hand and one group having practice trials with the left foot.

Whether a subject who had practice trials with the left foot should be placed in Group E, F, or J, was decided by his speed on the practice trials, in relation to the average speed of the members of each group already tested. The practice trials for Subject P, let us say, showed somewhat faster time records than the average subject. I then glanced at the average for each (partial) group and placed P in the slowest of the three groups. A similar procedure was carried out with subjects who had right hand practice. Since but a short time elapsed between practice and transfer trials, a few seconds was all that could be given to the choice of the appropriate group for

each subject, and only a fair measure of accuracy could be expected. My chief aim was to avoid giving any advantage to 'opposite and symmetrical' and 'same side' muscle groups, since these had shown greater transfer after 100 trials practice than the 'opposite and unsymmetrical.' The result was that I weighted the groups slightly in the opposite direction. From Table III and Fig. 5 it is clear that those groups which

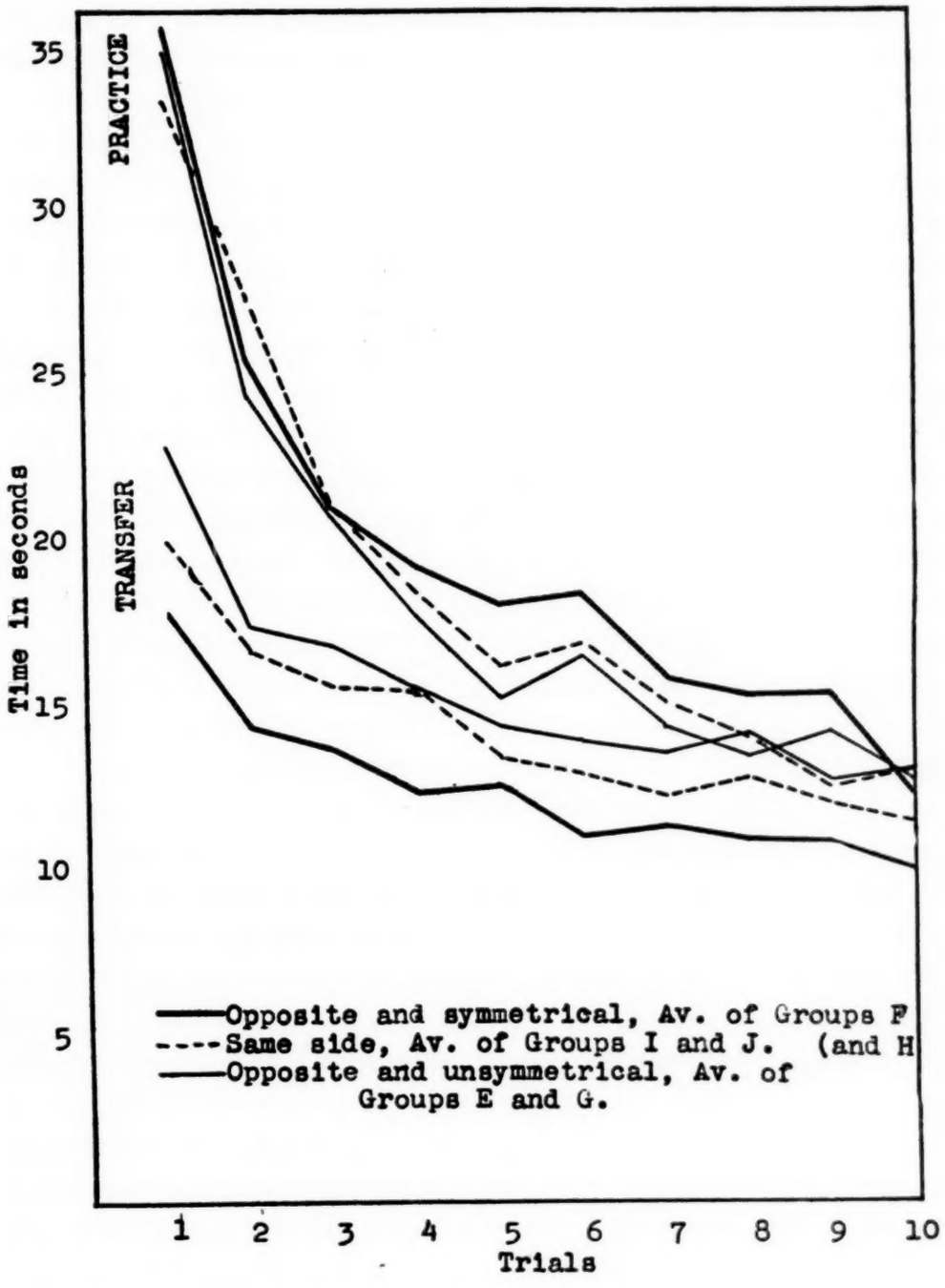

FIG. 5. Transfer to different muscle groups from 10 trials practice.

afterwards took transfer trials with the opposite and unsymmetrical muscle group make the best speed in the practice trials, the groups later tested for transfer to the same side of the body rank next, while the 'opposite and symmetrical' groups are slowest of the three in the practice trials.

The pairs of groups which were averaged to balance right-left and hand-foot differences were: (1) Opposite and symmetrical, Group F (left foot practice, right foot transfer) and Group H (right hand practice, left hand transfer); (2) Same side, Group J (left foot practice, left hand transfer) and Group I (right hand practice, right foot transfer); (3) Opposite and unsymmetrical, Group E (left practice, right hand transfer) and Group G (right hand practice, left foot transfer). Averages of the practice trials for these three pairs are shown by the upper curves in Fig. 5, and their transfer trials by the three lower curves in the same figure. The relation of the transfer groups to the practiced right hand or left foot is signified in Fig. 5 as follows: opposite and symmetrical, heavy solid lines; same side, dotted lines; opposite and unsymmetrical, light solid lines. Each practice curve is paralled by a transfer curve for the average of the same two groups. As noted in the last paragraph, in the practice trials the 'opposite and symmetrical' (transfer) groups are the slowest, the 'same side' next in speed, and the 'opposite and unsymmetrical' the fastest of the three. The relative speed of these three pairs of groups in the practice trials is reversed in the transfer trials.

According to the other method of equating the groups, by speed on trials without the mirror, we find some differences between individual groups, but close agreement between the pairs of groups mentioned above. The average time for hand and foot with direct view of the maze, computed as was done earlier for Groups A, B, C, and D (by weighting the hand records fifty percent and averaging hand and foot records), is:

Group F, 6.12 seconds, H, 6.40 seconds, average, 6.26 seconds;
Group J, 6.18 seconds, I, 6.40 seconds, average, 6.29 seconds;
Group E, 6.41 seconds, G, 6.17 seconds, average, 6.29 seconds.

In amount of transfer, the relative standing of the muscle

groups, as shown by the lower group of curves in Fig. 5, is the same as after 100 trials practice in Fig. 2. The heavy solid line is the lowest of the transfer curves, showing greatest transfer in Groups F and H, who used opposite and symmetrical (to the practiced) muscle groups in the transfer trials. The intermediate transfer (dotted) curve for Groups J and I signifies an intermediate amount of transfer to the muscle group on the same side as the one practiced. The uppermost of the transfer curves, for Groups E and G, indicates least transfer to the opposite and unsymmetrical right hand and left foot. Measured as before, in time saved on the first transfer trial, the transfer to the different muscle groups is: opposite and symmetrical, 77 percent; same side, 66 percent; opposite and unsymmetrical, 55 percent. These percentages are lower than the corresponding data from 100 trials practice, which showed a gain on the first transfer trial for the three muscle groups of 88 percent, 81 percent, and 74 percent respectively.

If we accept the evidence of the three pairs of groups, as seen in Fig. 5, and compare the latter with Fig. 2, the differences between the transfer curves are about the same after 10 trials and 100 trials practice. The latter, however, show less group variability. Following 100 trials practice the transfer from left foot to left hand was greater than the transfer from left foot to right hand for Group B and Group C. Similarly, the transfer from 100 trials practice with the right hand was greater to right foot than left foot for both Group A and Group D. In this comparison of transfer to the same side and opposite and unsymmetrical muscle groups the results from ten trials practice are not so consistent. In Fig. 4 we see that after 10 trials practice the left hand (Group J) is faster than the right hand (Group E) on the transfer trials. This agrees with our general results in showing greater transfer to the same side than to the opposite and unsymmetrical muscle group. On the other hand, after right hand practice, the right foot and left foot records (Group I and Group G, Fig. 4) are little different. There is some evidence, however, that Group G ranks above Group I in ability. The former

makes an average of one second better time in the practice trials in mirror-tracing (Table III) and is one-half second faster in the trials without the mirror. It may be that the superiority of Group G is masking a real advantage of right foot over left foot after 10 practice trials with the right hand. On the whole, however, the evidence that transfer is greatest to the opposite and symmetrical, next to the same side, and least to the opposite and unsymmetrical muscle group, is not so convincing after 10 trials practice as after 100 trials practice.

Permanence of the Transfer

It is clear from Fig. 4 and Fig. 5 that some transfer from ten trials practice persists for at least ten transfer trials, both to the opposite and symmetrical hand or foot and to the foot or hand on the same side of the body as the one practiced. The 'J' curve in Fig. 4, denoting transfer trials with the left hand after left foot practice, and the 'I' curve in the same figure, showing right foot transfer trials after right hand practice, were taken from trials with the foot or hand on the same side of the body as the practiced hand or foot. Both are lower at the end of ten transfer trials than the corresponding right hand or left foot practice curve for sixty-five subjects. Similarly, the upper broken line in Fig. 5, standing for the average of the right hand and left foot practice trials for Group J and Group I, is higher throughout than the lower broken line representing the average of ten transfer trials for the right foot and left hand of the same groups.

The groups which took transfer trials with the opposite and symmetrical muscle group give similar, but more marked results. The transfer data on left hand following right hand practice and right foot after left foot practice are not given in Fig. 4, since that figure is chiefly intended to show the relation between hand to foot and foot to hand transfer. However, Table III shows that at the end of ten trials the transfer series for both Group F and Group H are considerably lower than the corresponding practice series for sixty-five subjects plotted in Fig. 4. In Fig. 5, the same fact is demonstrated by the difference between the end points of the heavy solid lines,

the upper representing the practice series with right hand and left foot, the lower the transfer series for the left hand and right foot, Groups F and H.

For the groups which took transfer trials with the opposite and unsymmetrical muscle groups the results are less decisive. The 'E' curve (right hand after left foot) in Fig. 4 is below the right hand practice curve at the end of ten trials, but the 'G' curve (left foot after right hand) is higher than the left foot practice curve at the eighth and again at the tenth trial. Also, the lighter solid lines in Fig. 5, indicating practice and transfer trials for opposite and unsymmetrical muscle groups, differ little after the seventh trial. Our results, therefore, only justify the conclusion that transfer from ten trials practice in mirror tracing persists for seven (transfer) trials with the opposite and unsymmetrical muscle group.

Summary

The following summary includes the experiment previously reported and the present investigation.

Transfer to left hand, right foot, and left foot after right hand practice in mirror tracing a star-shaped maze, and to right foot, left hand, and right hand after left foot practice, was tested with 40 subjects for 100 practice trials, and 90 subjects for 10 practice trials. Forty of the last-named 90 subjects were given 20 additional trials with other muscle groups. The results may be outlined:

1. There is a large amount of transfer to all muscle groups tested, for 10, 20, 30, and 100 trials practice.

2. The transfer is a function of the whole practice curve. The amount of transfer is (roughly) proportional to the amount learned during the first ten, tenth to twentieth, twentieth to thirtieth, and the last seventy practice trials. Corresponding to the negative acceleration of the practice curve, the amount transferred is proportionately less from each successive division of the practice trials.

3. The transfer is greatest to the muscle group opposite and symmetrical, next to the same side, and least to the muscle group opposite and unsymmetrical to the practiced

hand or foot. The percentage of total-improvement-in-speed-on-the-practice-trials saved on the first transfer trial, is, after 10 practice trials, opposite and symmetrical muscle group, 77 percent, same side, 66 percent, opposite and unsymmetrical, 55 percent; after 100 trials practice, opposite and symmetrical, 89 percent, same side, 81 percent, opposite and unsymmetrical, 74 percent.

4. After 10 trials practice, transfer to the opposite and symmetrical and same side muscle groups is still effective at the end of 10 transfer trials. Transfer to the opposite and unsymmetrical hand or foot is doubtful for more than seven transfer trials after 10 trials practice, and 50 transfer trials after 100 trials practice.

5. There is no significant difference between the transfer from hand to foot and foot to hand.

(Manuscript received July 20, 1932)

SOME RECORDS OF THE MEMORIZING OF SONNETS

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Common sense tells us that it is easier to memorize material which has meaning for us, rather than to memorize that which is meaningless or that which we do not understand. And many psychological researches have confirmed this opinion. A brief illustration may be supplied by comparing two lines, and trying to repeat each of them after one reading apiece. The first line is:

"Many diamonds scintillate." (23 letters)

The second line is:

"Ua mau ke ea o ke aina i ka pono." (23 letters)

It is safe to say that any normal English-speaking adult can repeat the first line without any effort. It is far from likely that he can repeat the second line (unless he knows the Hawaiian language). Yet the alphabet is the same and the number of letters the same.

Although 'meaning' is a very genuine factor in the memorizing process, it is a factor for which we have no entirely satisfactory definition, and for which we have no adequate measure. An effort to control this feature of the memorizing process was made by Ebbinghaus when he devised his nonsense syllables. These syllables provided for psychologists a type of material to be memorized which was supposed to have a minimum of meaning, and hence to offer memoranda which should be plentiful in quantity and uniform in quality. Many valuable researches have been stimulated by this means. But the method has serious limitations, just because it does eliminate meaning. We shall never have a complete account of memorizing so long as we leave out of our experiments the very

factor which is most effective in bringing about that memorizing. We need to know more about the memorizing of particular instances of meaningful material. This paper aims to be a contribution to that end.

The only definition of meaning which is attempted here is a definition by denotation. We shall define meaning as that which is absent from a line like this:

"NOL ZIM RUF DOX RAD BUL MEF JOX KAV FEG"

and that which is present in a line like this:

"DEVOURING TIME, BLUNT THOU THE LION'S PAWS."

It must be acknowledged that the same line will have different meanings for different persons. Moreover, the same line may be said to represent different levels or degrees of meaning. There are those, for example, for whom Shakespeare's sonnets are little more than nonsense syllables. Whereas there are others for whom his sonnets represent the highest level of significance. Now, while we recognize these personal variations, we must also recognize that the meaning of a sonnet of Shakespeare is not entirely an individual affair with the reader. The sonnets have a life which is independent of any single generation of readers. They hold a position of great prestige in our literature. Hence their significance, if not completely objective, is something more than merely subjective.

In the present study ten of Shakespeare's sonnets were used as material to be memorized. Two questions confront us here. Are these sonnets equal to one another in degree of meaning? Are they equal to one another in degree of difficulty? We have no exact answer to these questions. The sonnets are equal in length, and they are similar in structure. Their formal elements are therefore as closely comparable as can well be expected of different selections. Whether they are equal in difficulty can best be known by finding out how long it takes to learn them. No effort will be made at this

time to estimate the degrees of meaning which the several sonnets represent.

Students, working in pairs, read the sonnets aloud to one another. The experimenter read sonnet 'A' (Shakespeare's 59th) aloud once. The subject then wrote down all that he could recall. The experimenter then read the same sonnet through again, and the subject, without referring to his first trial, wrote all that he could recall. This procedure continued until the sonnet had been heard five times, and five reproductions had been written. These results were scored according to the following rules. (1) One point credit is given for each word correctly written, except that (2) a half point is deducted for a word misplaced. But (3) if a whole sentence is correctly given but misplaced as a whole, then the penalty is not a half point for each word, but merely a whole point for the sentence. Experimenters were asked to do the best they could with these rules, using their own judgment if cases came up which seemed not to be covered by them, and to remember that they were trying to make a fair numerical estimate of the amount reproduced in each instance.

Following are the scores for each trial of each subject who memorized the sonnets. These are presented in Tables 1

TABLE 1

A. SHAKESPEARE'S SONNET LIX

"If there be nothing new, but that which is," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Hy.....	5	15	10	27	50
Du.....	16	14	21	29	26
Ma.....	4	11	18	48	74
Gr.....	9	25	37	47	57
Wy.....	17	42	48	71	88
Sc.....	12	25	29	36	38
Wa.....	8	26	27	41	54
Mc.....	5	26	25	48	70
Sm.....	17	33	45	53	61
Ke.....	1	3	15	22	24
Ed.....	8	19	13	19	21
Go.....	14	21	20	25	44
Mi.....	8	9	22	39	60
Median.....	8	21	22	39	54
Mean.....	9.1	20.7	25.3	38.8	51.3

TABLE 2

B. SHAKESPEARE'S SONNET XXIX

"When, in disgrace with fortune and men's eyes," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Hy.	15	20	34	37	43
Du.	9	21	20	29	39
Ma.	14	35	47	63	70
Gr.	22	40	46	63	69
Wy.	34	65	84	114	114
Sc.	17	18	24	34	42
Wa.	12	34	42	59	68
Mc.	9	15	35	45	65
Sm.	23	41	67	88	96
Ke.	11	19	25	34	52
Ed.	12	10	13	23	31
Go.	6	19	24	31	41
Mi.	17	41	49	77	79
Median.	14	21	35	45	65
Mean.	15.4	29.0	39.1	53.6	62.2

TABLE 3

C. SHAKESPEARE'S SONNET LXXIII

"That time of year thou mayst in me behold," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Hy.	10	22	29	36	48
Du.	11	23	19	27	27
Ma.	4	12	17	26	50
Gr.	24	40	58	60	73
Wy.	30	41	37	63	66
Sc.	12	23	27	36	40
Wa.	16	30	33	49	51
Mc.	6	25	31	40	59
Sm.	20	34	42	53	68
Ke.	13	22	33	41	46
Ed.	13	15	27	30	36
Go.	22	37	26	45	56
Mi.	50	81	103	108	108
Median.	13	25	31	41	51
Mean.	17.7	31.1	37.0	47.2	56.0

TABLE 4

D. SHAKESPEARE'S SONNET CXVI

"Let me not to the marriage of true minds," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Hy.	10	25	28	39	42
Du.	16	19	32	33	39
Ma.	33	55	68	90	111
Gr.	35	52	60	76	82
Wy.	27	34	60	67	92
Sc.	22	30	38	42	45
Wa.	20	29	36	50	61
Mc.	15	27	38	54	73
Sm.	23	48	69	88	96
Ke.	15	17	33	43	49
Ed.	20	32	37	44	53
Go.	27	31	46	50	62
Mi.	25	43	60	73	87
Median.	22	31	38	50	62
Mean.	22.1	34.0	46.5	57.6	68.6

TABLE 5

E. SHAKESPEARE'S SONNET XIX

"Devouring Time, blunt thou the lion's paws," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Hy.	10	21	31	33	36
Du.	25	46	49	57	71
Ma.	12	39	57	70	87
Gr.	26	66	73	75	92
Wy.	43	76	82	96	106
Sc.	21	41	45	53	54
Wa.	23	31	49	55	69
Mc.	20	39	60	70	78
Sm.	22	43	51	74	100
Ke.	10	28	46	61	72
Ed.	14	24	37	44	53
Go.	30	33	47	51	63
Mi.	11	40	53	76	82
Median.	21	39	49	61	72
Mean.	20.5	40.5	52.3	62.6	74.0

TABLE 6

I. SHAKESPEARE'S SONNET XVIII

"Shall I compare thee to a summer's day?" etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Br.....	15	18	19	33	39
Fr.....	9	30	40	38	51
Gr.....	24	21	42	54	68
He.....	16	37	50	65	87
Ma.....	19	24	46	65	71
Ro.....	12	32	47	58	67
Wi.....	10	18	23	32	38
Median.....	15	24	42	54	67
Mean.....	15	25.7	38.1	49.2	60.1

TABLE 7

II. SHAKESPEARE'S SONNET LXXXVIII

"Farewell! thou art too dear for my possessing," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Br.....	12	21	24	30	43
Fr.....	18	27	41	54	53
Gr.....	13	22	35	47	52
He.....	16	26	40	54	57
Ma.....	20	22	20	35	48
Ro.....	14	21	30	41	65
Wi.....	14	18	14	22	29
Median.....	14	22	30	41	52
Mean.....	15.2	22.4	29.1	40.4	49.5

TABLE 8

III. SHAKESPEARE'S SONNET XXX

"When to the sessions of sweet silent thought," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Br.....	16	8	16	30	30
Fr.....	22	29	40	56	47
Gr.....	25	37	49	55	73
He.....	14	34	50	63	68
Ma.....	26	43	48	71	87
Ro.....	20	27	42	40	54
Wi.....	2	27	34	39	46
Median.....	20	29	42	55	54
Mean.....	17.8	29.2	39.8	50.5	57.8

TABLE 9

IV. SHAKESPEARE'S SONNET LV

"Not marble, nor the gilded monuments," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Br.....	13	18	23	28	47
Fr.....	19	21	33	41	54
Gr.....	26	32	39	50	63
He.....	14	42	63	80	88
Ma.....	14	25	36	41	48
Ro.....	23	33	40	49	54
Wi.....	11	29	35	31	40
Median.....	14	29	36	41	54
Mean.....	17.1	28.5	38.4	45.7	56.2

TABLE 10

V. SHAKESPEARE'S SONNET XXVII

"Weary with toil, I haste me to my bed," etc.

Persons	Scores on Five Trials				
	I	II	III	IV	V
Br.....	20	24	19	25	30
Fr.....	30	41	62	69	68
Gr.....	20	33	44	54	80
He.....	20	33	57	85	101
Ma.....	20	28	41	56	65
Ro.....	23	39	41	49	60
Wi.....	15	21	30	32	34
Median.....	20	33	41	54	65
Mean.....	21.1	31.2	42	52.8	62.5

to 10. In Table 11 we have summarized the results of the thirteen persons who studied sonnets *A*, *B*, *C*, *D*, and *E*. These mean scores give a picture of the improvement which took place with practice. This improvement is in two directions. First there is the fact that the fifth trial is very much better than the first, in the case of the same sonnet. This is, of course, universally expected. But second there is a manifest improvement as we go from sonnet *A* to sonnet *E*, that is from the first one attempted to the last one attempted by each subject. In Table 12 we have the summary of results for the

TABLE 11

MEAN SCORE OF 13 PERSONS ON FIVE TRIALS

Trial	I	II	III	IV	V
Sonnet *A*	9.1	20.7	25.3	38.8	51.3
Sonnet *B*	15.4	29.0	39.1	53.6	62.2
Sonnet *C*	17.7	31.1	37.0	47.2	56.0
Sonnet *D*	22.1	34.0	46.5	57.6	68.6
Sonnet *E*	20.5	40.5	52.3	62.6	74.0

TABLE 12

MEAN SCORE OF 7 PERSONS ON FIVE TRIALS

Trial	I	II	III	IV	V
Sonnet I	15	25.7	38.1	49.2	60.1
Sonnet II	15.2	22.4	29.1	40.4	49.5
Sonnet III	17.8	29.2	39.8	50.5	57.8
Sonnet IV	17.1	28.5	38.4	45.7	56.2
Sonnet V	21.1	31.2	42.0	52.8	62.5

seven persons who studied sonnets I, II, III, IV, and V. Here too we find the same types of improvement, though it is less striking than the results of Table 11.

In order to complete the inquiry in a logically satisfactory manner the following steps should be taken. The five sonnets should be learned by (1) a group of persons who should take them in the order '*B*, *C*, *D*, *E*, *A*,' (2) a group of persons who should take them in the order '*C*, *D*, *E*, *A*, *B*,' (3) a group who should take them in the order '*D*, *E*, *A*, *B*, *C*,' and (4) a group who should take them in the order '*E*, *A*, *B*, *C*, *D*,' With these comparisons available we should be in position to judge whether the improvement is due to the meaning difficulty of the individual sonnet or whether it is a function of the position in the practice series.

(Manuscript received June 12, 1933)

AN EXPERIMENTAL STUDY OF COMPOSER-PREFERENCES OF FOUR OUTSTANDING SYMPHONY ORCHESTRAS [1]

BY EMIL E. E. FOLGMANN
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A new method for measuring social attitudes,[2] recently devised by Thurstone, has stimulated other investigators to apply Thurstone's method to the study of attitudes such as religion,[3] pacifism,[4] prohibition,[5] and nationality preferences.

In the present study of composer-preferences, the writer, formerly a member of both the Boston Symphony and the Philadelphia Orchestras, has made use of Thurstone's 'Law of Comparative Judgment' [6] in order to measure quantitatively the preferences for the music of different composers. In the formulation of this experiment, the members of four symphony orchestras were selected as subjects. With regard to the problem itself, scales of preferences were derived not only from the total statistical data of all four orchestras, but also from each orchestra separately. This accomplished, the different orchestral groups were then compared upon the bases of the scales derived.

Procedure

In general, the procedure used has been that employed

[1] The writer wishes to express his gratitude to Professor Samuel W. Fernberger of the University of Pennsylvania for the great interest shown in this study, and valuable suggestions. To Dr. Francis W. Irwin for suggesting the problem and for his ever-ready helpfulness, to Dr. Leon Arons for his constructive criticism and generous outlay of time in assisting in treating the results, the writer also acknowledges his indebtedness,

[2] For a summary and analysis of methods which have been used in studying attitudes, see Droba, D.D., Methods for measuring attitudes, *Psychol. Bull.*, 1932, **29**-309–323.

[3] Chave, E. J. and Thurstone, L.L., A Scale for Measuring Attitude toward God, Chicago: Univ. Chic. Press, 1931.

[4] Droba, D.D., A scale of militarism-pacifism, *J. Educ. Psychol.*, 1931, **22**, 96–111.

[5] Smith, Hattie N., A scale for measuring attitudes about prohibition, *J. Abn. & Soc. Psychol.*, 1932, **26**, 429–437.

[6] Thurstone, L.L., A law of comparative judgment, *Psychol. Rev.*, 1927, **34**, 273–286.

by Thurstone in his study of nationality preferences.[7] The composers to be used in the present study were determined by a preliminary experiment in which 50 music lovers were presented with a list of 40 composers. The Ss (Subjects) were asked to identify, where possible, each composer by giving some composition and several facts concerning the composer in question. Two fictitious names were included as controls. The results demonstrated that certain modern composers were not known to the Ss; and that there was some confusion between the names of several, as, for example, Richard Strauss and Johann Strauss. There was no identification offered for the fictitious names. Composers not well known to the Ss, or those apt to be confused, were not used in the final list. The list was designed so that it might be used, for comparative purposes, with groups other than professional musicians. The writer realizes that his list of composers is not an exhaustive one. However, he feels that it gives an acceptable cross-section of composers of music, and should furnish a valid basis for a scale of composer-preferences.[8] The final list includes 19 composers. These will be found in the tables.

A test schedule was prepared, consisting of:

(*a*) *Instructions*

"This is an experimental study of preferences of the music of different composers. You are asked to underline the name of the one composer of each pair whose *music you prefer* in general, *not* taking the personality or greatness of the composer into consideration.

For example, take the pair:

Puccini—Gounod.

If in general you prefer the music of Puccini to that of Gounod, underline *Puccini*, if on the other hand you prefer the music of Gounod, underline *Gounod*. To make this experiment valid, it is absolutely necessary not to omit any pair, even if it is difficult to make a choice.

[7] A detailed description of the method will be found in: Thurstone, L.L., An experimental study of nationality preferences, *J. Gen. Psychol.*, 1928, 1, 405–425.

[8] It might be well to point out here that the writer has an adequate musical background to bring into service for this study. He is a graduate of the *Royal Academy of Music of Berlin, Germany*, and studied with Professor Robert Haussmann and Professor Hugo Becker. He also has appeared both as a soloist and in chamber music ensemble here and abroad, and, for more than twenty years, has played with symphony orchestras—the Boston Symphony, the National Symphony Orchestra of New York, and the Philadelphia Orchestra. In addition, he has been a member of the faculty of the Department of Music of the University of Pennsylvania.

It is also of the greatest importance not to discuss the experiment with anyone before its completion."

(*b*) 190 pairs made from the list of composers, each composer being paired with every other one. Nineteen of the pairs were duplicates, and were used for 'control.' All pairs were chosen at random.

(*c*) A request for information was made concerning nationality, instrument, etc.

Important:

The following information is requested:

(Name is not necessary) Who was your teacher?.....
What was his nationality?.....

Citizenship:.....
Country of birth:.....
Birth place of father:.....
Birth place of mother:.....
Age:.....
Sex:.....
I play the following instrument:.....
Where did you receive your musical education?.....

It will be noted the name of the musician was not required. This anonymity, the writer feels, was conducive to complete frankness on the part of the Ss.

The schedule was multigraphed on three $8\frac{1}{2}'' \times 11''$ sheets, and was given to the members of four outstanding symphony orchestras, 308 completed schedules were secured.[9] Incomplete records were discarded. Of the 308 Ss, 95 are members of the Philadelphia Orchestra; 75, of the Philharmonic-Symphony Society of New York; 66, of the Boston Symphony Orchestra; and 72, of the Minneapolis Symphony Orchestra. The experiment was begun early in 1932.

Results

The results, both for the individual orchestras and for all taken together, have been treated according to Thurstone's Case V.[10]

The experimental proportions will be found in Tables 1–5. These proportions were derived by dividing the frequency of judgments in a given category, by the total number of judgments.

[9] The writer wishes to express his thanks to the members of the different orchestras for their generous cooperation. He is particularly grateful to Mr. Eugene Ormandy, the Conductor of the Minneapolis Symphony Orchestra, for his kindness in collecting the completed schedules.

[10] See footnote 7.

TABLE 1

EXPERIMENTAL PROPORTIONS PHILADELPHIA ORCHESTRA

	Bach	Beethoven	Berlioz	Brahms	Chopin	Debussy	MacDowell	Cesar Franck	Grieg	Haydn	Vic. Herbert	Mendelssohn	Mozart	Schubert	Schumann	Strawinsky	Tschaikowsky	Verdi	Wagner	Check Column
Bach	—	.453	.074	.432	.063	.179	.032	.053	.032	.126	.021	.053	.389	.179	.137	.084	.095	.074	.274	2.750
Beethoven	.547	—	.084	.432	.074	.168	.021	.063	.042	.105	.032	.053	.274	.105	.063	.116	.063	.074	.284	2.600
Berlioz	.926	.916	—	.968	.484	.895	.126	.547	.274	.821	.158	.653	.947	.884	.726	.589	.621	.495	.937	11.967
Brahms	.568	.568	.032	—	.063	.137	.032	.042	.011	.211	.021	.053	.358	.147	.074	.053	.084	.074	.368	2.896
Chopin	.937	.926	.516	.937	—	.726	.242	.568	.284	.800	.095	.589	.926	.895	.695	.568	.642	.484	.874	11.704
Debussy	.821	.832	.105	.863	.274	—	.053	.053	.063	.558	.053	.295	.811	.642	.347	.221	.263	.189	.758	7.201
MacDowell	.968	.979	.874	.968	.758	.947	—	.916	.800	.916	.379	.916	.989	1.000	.958	.821	.905	.874	.979	15.947
Cesar Franck	.947	.937	.453	.958	.432	.947	.084	—	.242	.842	.126	.568	.916	.884	.747	.589	.589	.411	.926	11.598
Grieg	.968	.958	.726	.989	.716	.937	.200	.758	—	.895	.189	.853	.968	.989	.895	.800	.842	.758	.989	14.430
Haydn	.874	.895	.179	.789	.200	.442	.084	.158	.105	—	.053	.179	.853	.547	.263	.158	.232	.126	.663	6.800
Vic. Herbert	.979	.968	.842	.979	.905	.947	.621	.874	.811	.947	—	.937	.968	.958	.958	.800	.884	.853	.989	16.220
Mendelssohn	.947	.947	.347	.947	.411	.705	.084	.432	.147	.821	.063	—	.916	.853	.632	.547	.484	.337	.884	10.504
Mozart	.611	.726	.053	.642	.074	.189	.011	.084	.032	.147	.032	.084	—	.189	.095	.074	.126	.053	.453	3.675
Schubert	.821	.895	.116	.853	.105	.358	.000	.116	.011	.453	.042	.147	.811	—	.200	.211	.189	.084	.716	6.128
Schumann	.863	.937	.274	.926	.305	.653	.042	.253	.105	.737	.042	.368	.905	.800	—	.389	.368	.232	.811	9.010
Strawinsky	.916	.884	.411	.947	.432	.779	.179	.411	.200	.842	.200	.453	.926	.789	.611	—	.568	.400	.874	10.822
Tschaikowsky	.905	.937	.379	.916	.358	.737	.095	.411	.158	.768	.116	.516	.874	.811	.632	.432	—	.326	.905	10.276
Verdi	.926	.926	.505	.926	.516	.811	.126	.589	.242	.874	.147	.663	.947	.916	.768	.600	.674	—	.947	12.103
Wagner	.726	.716	.063	.632	.126	.242	.021	.074	.011	.337	.011	.116	.547	.284	.189	.126	.095	.053	—	4.369
Σp	15.250	15.400	6.033	15.104	6.296	10.799	2.053	6.402	3.570	11.200	1.780	7.496	14.325	11.872	8.990	7.178	7.724	5.897	13.631	
Rank order	2	1	15	3	14	8	18	13	17	7	19	11	4	6	9	12	10	16	5	

TABLE 2

EXPERIMENTAL PROPORTIONS NEW YORK PHILHARMONIC

	Bach	Beethoven	Berlioz	Brahms	Chopin	Debussy	MacDowell	Cesar Franck	Grieg	Haydn	Vic. Herbert	Mendelssohn	Mozart	Schubert	Schumann	Strawinsky	Tschaikowsky	Verdi	Wagner	Check Column
Bach....	—	.693	.187	.467	.173	.373	.147	.120	.227	.333	.067	.253	.560	.347	.240	.080	.147	.187	.507	5.108
Beethoven.....	.307	—	.107	.227	.027	.093	.040	.040	.053	.107	.053	.107	.200	.133	.080	.067	.027	.120	.333	2.121
Berlioz.....	.813	.893	—	.907	.493	.747	.133	.293	.280	.840	.107	.653	.880	.867	.733	.253	.227	.453	.907	10.479
Brahms.....	.533	.773	.093	—	.120	.200	.013	.107	.067	.293	.053	.200	.480	.320	.213	.040	.080	.173	.533	4.291
Chopin.....	.827	.973	.507	.880	—	.760	.160	.507	.507	.867	.133	.747	.907	.893	.800	.307	.493	.533	.840	11.641
Debussy.....	.627	.907	.253	.800	.240	—	.080	.173	.227	.680	.067	.440	.813	.640	.467	.107	.173	.320	.747	7.761
MacDowell.....	.853	.960	.867	.987	.840	.920	—	.813	.813	.920	.587	.933	.920	.947	.933	.667	.733	.787	.973	15.453
Cesar Franck...	.880	.960	.707	.893	.493	.827	.187	—	.493	.800	.293	.760	.907	.853	.787	.413	.533	.573	.747	12.106
Grieg.....	.773	.947	.720	.933	.493	.773	.187	.507	—	.920	.200	.840	.947	.920	.800	.400	.560	.640	.920	12.480
Haydn.....	.667	.893	.160	.707	.133	.320	.080	.200	.080	—	.027	.347	.760	.533	.387	.093	.160	.160	.667	6.374
Vic. Hervert....	.933	.947	.893	.947	.867	.933	.413	.707	.800	.973	—	1.000	.933	.933	.907	.640	.787	.840	.947	15.400
Mendelssohn....	.747	.893	.347	.800	.253	.560	.067	.240	.160	.653	.000	—	.827	.733	.587	.173	.187	.320	.853	8.400
Mozart.....	.440	.800	.120	.520	.093	.187	.080	.093	.053	.240	.067	.173	—	.253	.227	.027	.093	.147	.507	4.120
Schubert.....	.653	.867	.133	.680	.107	.360	.053	.147	.080	.467	.067	.267	.747	—	.373	.053	.147	.147	.613	5.961
Schumann.....	.760	.920	.267	.787	.200	.533	.067	.213	.200	.613	.093	.413	.773	.627	—	.147	.173	.173	.800	7.759
Strawinsky.....	.920	.933	.747	.960	.693	.893	.333	.587	.600	.907	.360	.827	.973	.947	.853	—	.640	.600	.947	13.720
Tschaikowsky...	.853	.973	.773	.920	.507	.827	.267	.467	.440	.840	.213	.813	.907	.853	.827	.360	—	.533	.907	12.280
Verdi.....	.813	.880	.547	.827	.467	.680	.213	.427	.360	.840	.160	.680	.853	.853	.827	.400	.467	—	.827	11.121
Wagner.....	.493	.667	.093	.467	.160	.253	.027	.253	.080	.333	.053	.147	.493	.387	.200	.053	.093	.173	—	4.425
Σp.....	12.892	15.879	7.521	13.709	6.359	10.239	2.547	5.894	5.520	11.626	2.600	9.600	13.880	12.039	10.241	4.280	5.720	6.879	13.575	
Rank order.....	5	1	11	3	13	9	19	14	16	7	18	10	2	6	8	17	15	12	4	

TABLE 3

EXPERIMENTAL PROPORTIONS BOSTON SYMPHONY

	Bach	Beethoven	Berlioz	Brahms	Chopin	Debussy	MacDowell	Cesar Franck	Grieg	Haydn	Vic. Herbert	Mendelssohn	Mozart	Schubert	Schumann	Strawinsky	Tschaikowsky	Verdi	Wagner	Check Column
Bach	—	.651	.242	.379	.136	.273	.061	.288	.121	.288	.061	.182	.439	.242	.197	.182	.091	.152	.530	4.515
Beethoven	.349	—	.091	.197	.076	.152	.030	.076	.045	.030	.030	.015	.197	.045	.045	.091	.121	.045	.394	2.629
Berlioz	.758	.909	—	.742	.333	.651	.076	.333	.242	.697	.091	.409	.803	.636	.621	.303	.439	.364	.818	9.225
Brahms	.621	.803	.258	—	.182	.333	.045	.273	.136	.258	.045	.106	.545	.227	.288	.212	.136	.106	.621	5.195
Chopin	.864	.924	.667	.818	—	.758	.197	.667	.455	.712	.182	.576	.712	.803	.682	.500	.576	.545	.864	11.502
Debussy	.727	.848	.349	.667	.242	—	.045	.227	.167	.591	.076	.333	.712	.545	.409	.152	.318	.318	.742	7.468
MacDowell	.939	.970	.924	.955	.803	.955	—	.879	.833	.955	.333	.939	.924	.939	.970	.848	.909	.848	.970	15.893
Cesar Franck	.712	.924	.667	.727	.333	.773	.121	—	.273	.651	.076	.485	.788	.667	.636	.424	.500	.364	.833	9.954
Grieg	.879	.955	.758	.864	.545	.833	.167	.727	—	.818	.167	.682	.864	.879	.864	.576	.591	.591	.970	12.730
Haydn	.712	.970	.303	.742	.288	.409	.045	.349	.182	—	.030	.273	.833	.545	.439	.227	.258	.212	.788	7.605
Vic. Herbert	.939	.970	.909	.955	.818	.924	.667	.924	.833	.970	—	.894	.939	.939	.939	.818	.909	.894	.970	16.211
Mendelssohn	.818	.985	.591	.894	.424	.667	.061	.515	.318	.727	.106	—	.894	.803	.682	.455	.500	.439	.939	10.818
Mozart	.561	.803	.197	.455	.288	.288	.076	.212	.136	.167	.061	.106	—	.197	.212	.136	.121	.061	.591	4.668
Schubert	.758	.955	.364	.773	.197	.455	.061	.333	.121	.455	.061	.197	.803	—	.394	.242	.242	.242	.818	7.471
Schumann	.803	.955	.379	.712	.318	.591	.030	.364	.136	.561	.061	.318	.788	.606	—	.273	.333	.242	.818	8.288
Strawinsky	.818	.909	.697	.788	.500	.848	.152	.576	.424	.773	.182	.545	.864	.758	.727	—	.530	.455	.909	11.455
Tschaikowsky	.909	.879	.561	.864	.424	.682	.091	.500	.409	.742	.091	.500	.879	.758	.667	.470	—	.455	.894	10.775
Verdi	.848	.955	.636	.894	.455	.682	.152	.636	.409	.788	.106	.561	.939	.758	.758	.545	.545	—	.909	11.576
Wagner	.470	.606	.182	.379	.136	.258	.030	.167	.030	.212	.030	.061	.409	.182	.182	.091	.106	.091	—	3.622
Σp	13.485	15.371	8.775	12.805	6.498	10.532	2.107	8.046	5.270	10.395	1.789	7.182	13.332	10.529	9.712	6.545	7.225	6.424	14.378	
Rank order	3	1	10	5	15	6	18	11	17	8	19	13	4	7	9	14	12	16	2	

TABLE 4

EXPERIMENTAL PROPORTIONS MINNEAPOLIS SYMPHONY

	Bach	Beethoven	Berlioz	Brahms	Chopin	Debussy	MacDowell	Cesar Franck	Grieg	Haydn	Vic. Herbert	Mendelssohn	Mozart	Schubert	Schumann	Strawinsky	Tschaikowsky	Verdi	Wagner	Check Column
Bach	—	.722	.264	.611	.167	.333	.125	.222	.222	.306	.222	.292	.458	.417	.319	.236	.375	.222	.569	6.082
Beethoven	.278	—	.097	.444	.083	.097	.000	.056	.069	.042	.028	.056	.153	.125	.042	.083	.194	.042	.458	2.347
Berlioz	.736	.903	—	.958	.444	.514	.181	.472	.431	.694	.222	.708	.903	.819	.736	.333	.694	.319	.931	10.998
Brahms	.389	.556	.042	—	.097	.181	.014	.097	.069	.056	.042	.083	.236	.167	.111	.097	.139	.056	.375	2.807
Chopin	.833	.917	.556	.903	—	.681	.278	.528	.458	.736	.181	.750	.931	.875	.708	.403	.750	.361	.931	11.780
Debussy	.667	.903	.486	.819	.319	—	.125	.306	.319	.542	.125	.514	.792	.681	.500	.264	.514	.264	.792	8.932
MacDowell	.875	1.000	.819	.986	.722	.875	—	.792	.847	.903	.611	.944	.986	.986	.847	.583	.875	.722	1.000	15.373
Cesar Franck	.778	.944	.528	.903	.472	.694	.208	—	.458	.708	.250	.681	.861	.861	.750	.333	.694	.375	.931	11.429
Grieg	.778	.931	.569	.931	.542	.681	.153	.542	—	.708	.181	.778	.875	.875	.778	.403	.653	.417	.958	11.753
Haydn	.694	.958	.306	.944	.264	.458	.097	.292	.292	—	.111	.500	.917	.764	.528	.236	.486	.208	.833	8.888
Vic. Herbert	.778	.972	.778	.958	.819	.875	.389	.750	.819	.889	—	.917	.944	.972	.917	.556	.889	.667	.986	14.875
Mendelssohn	.708	.944	.292	.917	.250	.486	.056	.319	.222	.500	.083	—	.833	.722	.472	.264	.472	.181	.819	8.540
Mozart	.542	.847	.097	.764	.069	.208	.014	.139	.125	.083	.056	.167	—	.333	.181	.139	.292	.069	.583	4.708
Schubert	.583	.875	.181	.833	.125	.319	.014	.139	.125	.236	.028	.278	.667	—	.222	.111	.333	.097	.681	5.847
Schumann	.681	.958	.264	.889	.292	.500	.153	.250	.222	.472	.083	.528	.819	.778	—	.208	.458	.208	.819	8.582
Strawinsky	.764	.917	.667	.903	.597	.736	.417	.667	.597	.764	.444	.736	.861	.889	.792	—	.819	.542	.917	13.029
Tschaikowsky	.625	.806	.306	.861	.250	.486	.125	.306	.347	.514	.111	.528	.708	.667	.542	.181	—	.236	.875	8.474
Verdi	.778	.958	.681	.944	.639	.736	.278	.625	.583	.792	.333	.819	.931	.903	.792	.458	.764	—	.944	12.958
Wagner	.431	.542	.069	.625	.069	.208	.000	.069	.042	.167	.014	.181	.417	.319	.181	.083	.125	.056	—	3.598
Σp	11.918	15.653	7.002	15.193	6.220	9.068	2.627	6.571	6.247	9.112	3.125	9.460	13.292	12.153	9.418	4.971	9.526	5.042	14.402	
Rank order	6	1	12	2	15	11	19	13	14	10	18	8	4	5	9	17	7	16	3	

TABLE 5

EXPERIMENTAL PROPORTIONS TOTAL—ALL ORCHESTRAS

	Bach	Beethoven	Berlioz	Brahms	Chopin	Debussy	MacDowell	Cesar Franck	Grieg	Haydn	Vic. Herbert	Mendelssohn	Mozart	Schubert	Schumann	Strawinsky	Tschaikowsky	Verdi	Wagner	Check Column
Bach	—	.617	.182	.471	.183	.282	.088	.159	.143	.253	.088	.185	.458	.289	.218	.140	.172	.153	.455	4.536
Beethoven	.383	—	.094	.334	.065	.130	.023	.058	.052	.075	.036	.058	.211	.104	.058	.091	.097	.071	.360	2.300
Berlioz	.818	.906	—	.903	.445	.718	.127	.422	.305	.769	.146	.614	.890	.812	.708	.386	.536	.416	.903	10.824
Brahms	.529	.666	.097	—	.110	.205	.026	.120	.065	.205	.039	.107	.399	.211	.097	.094	.107	.101	.464	3.642
Chopin	.817	.935	.555	.890	—	.698	.221	.565	.416	.782	.143	.662	.877	.870	.721	.451	.617	.481	.877	11.578
Debussy	.718	.870	.282	.795	.302	—	.075	.179	.185	.591	.078	.390	.786	.630	.425	.188	.312	.266	.760	7.832
MacDowell	.912	.977	.873	.974	.779	.925	—	.854	.821	.922	.474	.932	.958	.971	.929	.734	.857	.812	.981	15.685
Cesar Franck	.841	.942	.578	.880	.435	.821	.146	—	.360	.760	.185	.623	.873	.825	.734	.451	.581	.432	.864	11.331
Grieg	.857	.948	.695	.935	.584	.815	.179	.640	—	.841	.185	.795	.919	.922	.838	.562	.675	.419	.961	12.770
Haydn	.747	.925	.231	.795	.218	.409	.078	.240	.159	—	.055	.315	.841	.594	.393	.182	.279	.172	.731	7.364
Vic. Herbert	.912	.964	.854	.961	.857	.922	.526	.815	.815	.945	—	.938	.948	.951	.932	.708	.867	.815	.974	15.704
Mendelssohn	.815	.942	.386	.893	.338	.610	.068	.377	.205	.685	.062	—	.970	.782	.594	.045	.412	.318	.873	9.375
Mozart	.542	.789	.110	.601	.123	.214	.042	.127	.081	.159	.052	.030	—	.240	.172	.091	.156	.081	.490	4.100
Schubert	.711	.896	.188	.789	.130	.370	.029	.175	.078	.406	.049	.218	.760	—	.289	.149	.224	.136	.705	6.302
Schumann	.782	.942	.292	.903	.279	.575	.071	.266	.162	.607	.068	.406	.828	.711	—	.263	.334	.214	.812	8.515
Strawinsky	.860	.909	.614	.906	.549	.812	.266	.549	.438	.818	.292	.955	.909	.851	.737	—	.640	.494	.909	12.508
Tschaikowsky	.828	.903	.464	.893	.383	.688	.143	.419	.325	.721	.133	.588	.844	.776	.666	.360	—	.383	.896	10.413
Verdi	.847	.929	.584	.899	.519	.734	.188	.568	.581	.828	.185	.682	.919	.864	.786	.506	.617	—	.909	12.145
Wagner	.545	.640	.097	.536	.123	.240	.019	.136	.039	.269	.026	.127	.510	.295	.188	.091	.104	.091	—	4.076
Σp	13.464	15.700	7.176	14.358	6.422	10.168	2.315	6.669	5.230	10.636	2.296	8.625	13.900	11.698	9.485	5.492	7.587	5.855	13.924	
Rank order	5	1	12	2	14	8	18	13	17	7	19	10	4	6	9	16	11	15	3	

TABLE 6

SCALE SEPARATIONS

Phila. Orchestra		New York Philharmonic		Boston Symph.		Minneapolis Symph.		Total	
Beethoven-Bach	.0532	Beethoven-Mozart	.5940	Beethoven-Wagner	.5005	Beethoven-Brahms	.1714	Beethoven-Brahms	.4268
Bach-Brahms	−.0266	Mozart-Brahms	.0557	Wagner-Bach	.3156	Brahms-Wagner	.2263	Brahms-Wagner	.1123
Brahms-Mozart	.2546	Brahms-Wagner	.0574	Bach-Mozart	.0052	Wagner-Mozart	.3120	Wagner-Mozart	−.0258
Mozart-Wagner	.2384	Wagner-Bach	.2059	Mozart-Brahms	.1169	Mozart-Schubert	.3336	Mozart-Bach	.2315
Wagner-Schubert	.5250	Bach-Schubert	.0886	Brahms-Debussy	.5143	Schubert-Bach	.0308	Bach-Schubert	.2986
Schubert-Haydn	.1639	Schubert-Haydn	.1430	Debussy-Schubert	.0402	Bach-Tschaikowsky	.4913	Schubert-Haydn	.3528
Haydn-Debussy	.0305	Haydn-Schumann	.3127	Schubert-Haydn	.0186	Tschaikowsky-Mendelssohn	.0261	Haydn-Debussy	.0908
Debussy-Schumann	.4712	Schumann-Debussy	.2158	Haydn-Schumann	.1325	Mendelssohn-Schumann	.0305	Debussy-Schumann	.1764
Schumann-Tschaikowsky	.3216	Debussy-Mendelssohn	.1446	Schumann-Berlioz	.2181	Schumann-Haydn	.0960	Schumann-Mendelssohn	.1972
Tschaikowsky-Mendelssohn	.0700	Mendelssohn-Berlioz	.5052	Berlioz-Franck	.1459	Haydn-Debussy	−.0499	Mendelssohn-Tschaikowsky	.2248
Mendelssohn-Strawinsky	.0759	Berlioz-Verdi	.1183	Franck-Tschaikowsky	.1972	Debussy-Berlioz	.5478	Tschaikowsky-Berlioz	.0960
Strawinsky-Franck	.2129	Verdi-Chopin	.1116	Tschaikowsky-Mendelssohn	−.0094	Berlioz-Franck	.0328	Berlioz-Franck	.1094
Franck-Chopin	−.0305	Chopin-Franck	.1045	Mendelssohn-Strawinsky	.1587	Franck-Grieg	.0774	Franck-Chopin	.0484
Chopin-Berlioz	.1027	Franck-Tschaikowsky	.0699	Strawinsky-Chopin	−.0030	Grieg-Chopin	.0223	Chopin-Strawinsky	.2464
Berlioz-Verdi	.0134	Tschaikowsky-Grieg	.0817	Chopin-Verdi	−.0097	Chopin-Verdi	.2650	Strawinsky-Verdi	−.1109
Verdi-Grieg	.6558	Grieg-Strawinsky	.3237	Verdi-Grieg	.3461	Verdi-Strawinsky	−.1124	Verdi-Grieg	.1920
Grieg-McDowell	.5536	Strawinsky-Herbert	.4145	Grieg-McDowell	.9311	Strawinsky-Herbert	.6143	Grieg-McDowell	.8815
McDowell-Herbert	.1077	Herbert-McDowell	.0085	McDowell-Herbert	.0432	Herbert-McDowell	.1091	McDowell-Herbert	.0358

In Table 1, the value .547 (column 2, row 3) is to be interpreted that 54.7 percent of the Ss preferred Bach to Beethoven. The values in the column headed 'Check Column' (at the complete right), represent the summation of the rows. The summation of the columns are at the bottom of the table in the row headed 'Σp.' The sum of the column and the row designated by any given composer, should be 18,000, thus providing a check on the operations. The Σp values have been ranked, and will be found in the bottom row ('Rank Order'). These rank orders are used in the determination of the scale separations.

TABLE 7

SCALE VALUES

	Phila. Orch.	New York Phil.	Boston Symph.	Minneapolis Symph.	Total
Bach	−0.0532	−0.9130	−0.8161	−1.0741	−0.7448
Beethoven	0.0000	0.0000	0.0000	0.0000	0.0000
Berlioz	−2.4628	−2.3229	−1.8619	−2.2159	−2.1814
Brahms	−0.0266	−0.6497	−0.9382	−0.1714	−0.4268
Chopin	−2.3601	−2.5528	−2.3513	−2.3484	−2.3392
Debussy	−1.2390	−1.6731	−1.4525	−1.6681	−1.4870
MacDowell	−3.6856	−3.5556	−3.6188	−3.2244	−3.5482
Cesar Franck	−2.3905	−2.6573	−2.0078	−2.2487	−2.2908
Grieg	−3.1320	−2.8089	−2.6877	−2.3261	−2.6667
Haydn	−1.2085	−1.1446	−1.5113	−1.7180	−1.3962
Victor Herbert	−3.7933	−3.5471	−3.6620	−3.1153	−3.5840
Mendelssohn	−2.1018	−1.8177	−2.1956	−1.5915	−1.8606
Mozart	−0.2812	−0.5940	−0.8213	−0.7097	−0.5133
Schubert	−1.0446	−1.0016	−1.4927	−1.0433	−1.0434
Schumann	−1.7102	−1.4573	−1.6438	−1.6220	−1.6634
Strawinsky	−2.1777	−3.1326	−2.3543	−2.5010	−2.5856
Tschaikowsky	−2.0318	−2.7272	−2.2050	−1.5654	−2.0854
Verdi	−2.4762	−2.4412	−2.3416	−2.6134	−2.4747
Wagner	−0.5196	−0.7071	−0.5005	−0.3977	−0.5391

The scale separations are given in Table 6. These separations were determined by using the formula: $S_1 - S_2 = x_{12}\sqrt{2}$.[11] To prepare the data for substitution in the above formula, it was necessary to convert the individual experimental proportions (Tables 1–5) to sigma values, sum these columns, and take the differences, the order in which the columns were taken being determined by the rank order value (Tables 1–5).

The scale values (Table 7) have been derived directly from

[11] See footnote 7, p. 407.

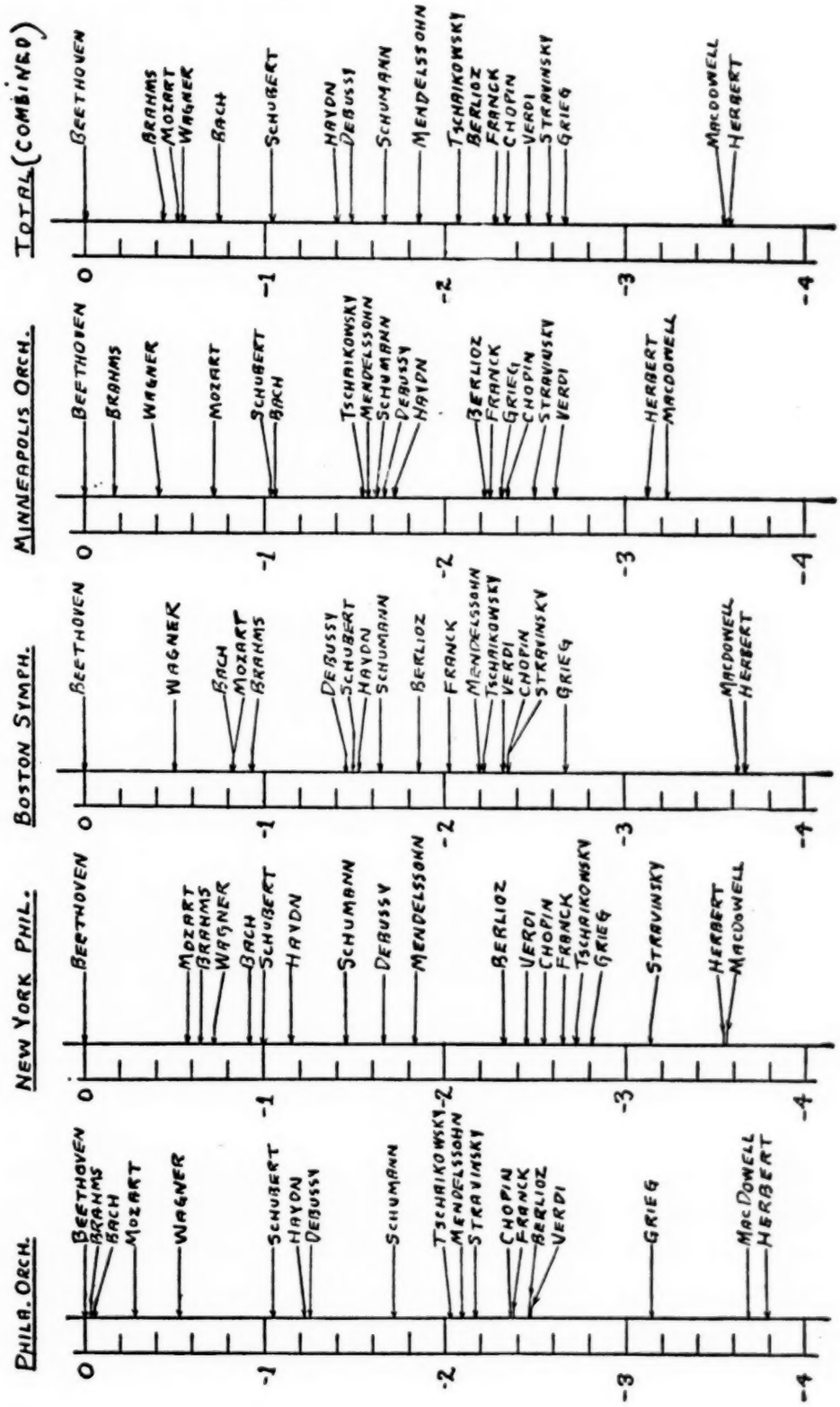

FIG. 1. Scales of Composer-Preferences.

the scale separations. Beethoven, ranking first for all the orchestras, has been taken arbitrarily as the origin (0.000). These scale values have been represented graphically in Fig. 1.

The rank orders of the various composers for the different orchestras have been collected in Table 8. These values have been taken from the scales, and with few exceptions are the same as are to be found in Tables 1–5. The discrepancies may be explained on the basis of changes brought about by the weighting accompanying the conversion of experimental proportions to sigma values.

TABLE 8

RANK ORDERS FOR THE DIFFERENT COMPOSERS

	Phila. Orch.	N. Y. Phil.	Bost. Symph.	Minn. Symph.	All Orch.
Bach	3	5	3	6	5
Beethoven	1	1	1	1	1
Berlioz	15	11	10	12	12
Brahms	2	3	5	2	2
Chopin	13	13	15	15	14
Debussy	8	9	6	10	8
MacDowell	18	19	18	19	18
Cesar Franck	14	14	11	13	13
Grieg	17	16	17	14	17
Haydn	7	7	8	11	7
Victor Herbert	19	18	19	18	19
Mendelssohn	11	10	12	8	10
Mozart	4	2	4	4	3
Schubert	6	6	7	5	6
Schumann	9	8	9	9	9
Strawinsky	12	17	16	16	16
Tschaikowsky	10	15	13	7	11
Verdi	16	12	14	17	15
Wagner	5	4	2	3	4

DISCUSSION

In examining the scales (Fig. 1), several things are immediately evident: Beethoven is always first; Schumann remains comparatively constant both as to rank order and scale value; and MacDowell and Herbert are consistently last. It will be remembered that Beethoven was taken as the origin of each scale. It is evident, therefore, that the scale values for the other composers in reality represent distance from Beethoven. Beethoven's scale value in itself gives little insight into the question of his relative strength in the four

orchestras. However, we may obtain indices of this strength by examining the Σp values (Tables 1–5) and noting the magnitude of the first scale separation (Table 6). The largest scale separation is that of the New York Philharmonic (.5940), the smallest is that of the Philadelphia Orchestra (.0266). In the case of the New York Philharmonic, the magnitude of this scale separation may indicate the greater relative strength of Beethoven in this orchestra, relative weakness of the immediately succeeding composers, or both. The Σp value (15.879), however, is reasonably higher for this orchestra than for the others, and it seems reasonable to conclude that Beethoven's strength is greatest in this orchestra. In this respect it is interesting to compare the Philadelphia and the Boston Symphony Orchestras. Although the Σp values are almost the same (15.400 and 15.371) yet there is a great difference in the size of the first scale separations (.0266 and .5005). It will be noted that the first scale separation for Boston is not appreciably smaller than that for the New York Philharmonic (.5005 and .5940). It would seem, as a result, that the scale value for Boston gives more of a line on the relative weakness of the next composer (Wagner), than on Beethoven's relative strength in this orchestra.

The Philadelphia Orchestra is distinctive in that while Beethoven is as strong here as in the other orchestras (except the New York Philharmonic), both Brahms and Bach are virtually of equal strength. This is easily evident from an examination of both the Σp and the scale values. The scale values for these three composers are 0.000, − 0.0266 and − 0.0532.

With the exception of the Minneapolis Symphony Orchestra, the scales are approximately of the same length. The scale for the New York Philharmonic has the most uniform distribution of values, indicating comparatively clean-cut preferences. On the other hand, clusterings of names will be found in the scales for the other orchestras.

A most interesting observation for the writer has been the position in the scale for the Philadelphia Orchestra of three composers: Bach, Debussy, and Stravinsky. These three are

higher in this scale than in that for any of the other orchestras. This may be due to the fact that the conductor, Dr. Leopold Stokowski, is himself a great admirer of the three composers. Especially can this be said of Bach. Many of his compositions have been orchestrated by Dr. Stokowski, and these arrangements, still in manuscript, are played only by the Philadelphia Orchestra. The Gothic structure and the substance of Bach's music are brought out more forcefully in these orchestrations. These have been greeted with tremendous enthusiasm both on the part of the musicians and the audiences. They are frequently performed, and it is highly possible that they have enhanced Bach's position with the orchestra membership. Many members of the orchestra have concurred with the writer in this opinion. With regard to Debussy it will be noted that his scale value for the Philadelphia Orchestra is higher than that for the Boston Symphony in spite of the fact that Debussy ranks sixth for Boston and eighth for Philadelphia.

Earlier, Beethoven's greater strength with the New York Philharmonic was indicated.[12] In this connection, Tschaikowsky's very low position in this orchestra also may be pointed out. Mr. Arturo Toscanini, recognized as a great interpreter of Beethoven, is known to have an aversion to Tschaikowsky. Under his leadership, Tschaikowsky is seldom played. It is possible that both Beethoven's greater strength and Tschaikowsky's low position in this orchestra are a reflection of the attitude of its forceful leader.

Undoubtedly, there may be a number of factors affecting preferences for the various composers. Among these may be listed the influence of the conductor, the nationalities represented in the orchestra membership, environmental factors such as location of the orchestra, extra-musical activities of its members, and the different instruments played.

[12] It is interesting to note here the following excerpt by Olin Downes, the eminent music critic, which appeared in *The New York Times*, of April 23, 1933: "The experiment of the cycle of Beethoven symphonies which has brought this series of concerts to an end has been extremely successful and has attracted audiences much larger than any earlier Sunday afternoon audiences of the Winter. This is partly a tribute, and an astonishing one, to the potency of Beethoven's music."

In order to gain an insight into the possible effect of nationality representation in a given orchestra, upon the preferences expressed, a table of nationality distribution has been prepared (Table 9). The criterion of nationality used was that

TABLE 9

NATIONALITY OF Ss IN PERCENTAGES

	Phila.	N. Y.	Bost.	Minn.	Total
American	22.1	5.3	15.2	34.7	19.5
French	3.2	6.7	27.3	6.9	10.1
German	21.1	22.7	18.2	29.2	22.7
Italian	10.5	16.0	4.5	0.0	8.1
Russian	34.7	34.7	15.2	2.8	23.1
Misc.	8.4	14.7	19.7	26.4	16.6
No. of cases	95	75	66	72	308

of the birthplace of the parents. A S, whose parents were born in the same country, was considered to be of that nationality. In the category of miscellaneous ('Misc.') have been included those Ss whose fathers and mothers were born in different countries. Nationalities appearing but infrequently also have been listed in this category.

The table reveals the highest percentage of French to be in the Boston Symphony, and it is interesting to note that Berlioz, Debussy and Franck are placed higher in the scale for this orchestra. Of the 26.4 percent of the membership of the Minneapolis Symphony listed under 'Misc.,' several are Scandinavian. In addition, it is highly possible that some of the 34.7 percent (listed under 'American' in the table) may be of Scandinavian descent. The extent to which the Scandinavian extraction of some of the orchestra members, may have been an important factor in Grieg's unusually high position in the scale for this orchestra, cannot at present be determined. The influence of nationality upon composer-preferences cannot be clearly ascertained from the data on hand. To remedy this factor the writer is planning a further investigation. The influence of the environmental factors listed earlier upon composer-preferences, is obscure and likewise awaits further experimentation.

With reference to the 'control' pairs mentioned in an

earlier section, it may be said that no statistically reliable difference was discovered between the experimental proportions gotten from these pairs and the corresponding experimental proportions which are to be found in Tables 1–5. The records of individual Ss may show shifts of attitude with regard to a given pair. For example, one S reported: "Take Wagner and Tschaikowsky. The first time the pair appeared, I thought of 'Parsifal' and underlined Wagner. The next time I came to the same pair (control pair) I thought of the 'Pathetique Symphony.' I like this symphony and I underlined Tschaikowsky." In view of the occasional shifting of attitude in individual records, it is reassuring that no statistically reliable difference in the experimental proportions resulted when the control pairs were considered, lending additional support to the adequacy of the method used, for a study of composer-preferences.

Summary and Conclusions

Thurston's Law of Comparative Judgment, Case V, was employed in a study of the composer-preferences of four symphony orchestras. Nineteen composers were used and data were secured from 308 Ss who made a total of 58,520 judgments. The scales were constructed and on that basis the orchestras compared. The results show Beethoven to be consistently first for all orchestras, and Herbert and MacDowell last. Schumann's position likewise is rather constant throughout. The remaining composers vary in position from orchestra to orchestra. With the exception of that for the Minneapolis Symphony Orchestra, the scales have approximately the same range. The influence of several factors such as nationality, the personality of the conductor and the location of the orchestra, upon the preferences expressed by the orchestras, has been suggested, and the need for further experimentation indicated.

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A STUDY OF THE DEVELOPMENT OF TOLERANCE FOR CAFFEINATED BEVERAGES[1]

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Ever since its widespread adoption as a beverage both the immediate and ultimate effects of coffee on the organism have been the object of a great deal of speculation and scientific investigation. Any analysis of these studies is impressive chiefly in the widespread disagreement found in the conclusions. Undoubtedly many of these conflicting findings may be attributed to preconceived prejudices of the experimenter or to a lack of proper control of the factors involved in the experiment. But there is a condition inherent in the problem itself which must be understood before the exact effect of coffee or any other caffeinated beverage may be determined, even under a controlled situation. This disturbing factor is the capacity of the human organism, under certain conditions, to develop a tolerance for caffeine. Once this tolerance is developed the character of the reaction of the organism to caffeine undergoes a change. By showing that habituated users of coffee exhibit a different type of reaction from that of the non-habituated, investigators have indicated the existence of this adaptive capacity but few if any attempts have been made to describe or measure its development. Until more is known of such a basic phenomenon, studies of the effect of coffee on behavior will continue to yield contradictory results. The following report represents an initial approach to a better understanding of this important process.

In order to observe the development of a change which apparently is very slow, it was necessary to use some organic activity as a control which lent itself readily to accurate measurement and which was definitely influenced by coffee.

[1] The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research established by August Heckscher at Cornell University.

Previous research with parotid secretion had indicated that the normal flow from either of these glands could be measured accurately (1). Furthermore, a preliminary test showed that the rate of secretion from these glands was definitely affected both directly and indirectly by the ingestion of coffee. That is, aside from the direct increase in flow due to oral stimulation and swallowing, there was a marked increase in secretion apparently due to the absorption of caffeine into the blood. This change appeared from ten to fifteen minutes after the ingestion of the beverage. This indirect effect on the rate of salivary secretion afforded an ideal activity for a controlled study of the development of tolerance. Presumably if the organism changed under the persistent stimulation of caffeine, the change would be reflected in secretions as well as in other functions. Theoretically variation in secretion from the direct oral stimulation could also be used to show these changes. But previous investigation had shown this reaction to be somewhat complicated by conditioned or psychic responses. Accordingly the secretory changes appearing after the ingestion of the beverage were selected as a control for our study.

The technique for the collection and measurement of the secretion from the parotid gland of human subjects has been described in detail in earlier studies (2). A small silver suction disc is fastened over the mouth of Stenson's duct. From the inner chamber of this disc a small airtight drainage tube passes through the corner of the mouth carrying the secretion to a measuring device capable of measuring the secretion in hundredths of a cc. The device itself does not affect the flow of secretion and does not hurt the subject or interfere with mouth movements.

The subjects employed for the experiments were male and female college students who were paid by the hour. In the preliminary studies, tests were made with habituated and non-habituated subjects to determine the characteristic reactions of the two groups, but in the prolonged studies subjects were selected who had not previously used any caffeinated beverages regularly. The experiments were conducted in a quiet room with only the experimenter and subject present.

The procedure adopted was simple. As soon as the disc was properly adjusted the subject assumed and maintained a comfortable posture. He was instructed to avoid excitatory movements of the tongue or throat, except as directed, and to keep the mouth in a natural closed position. At given intervals swallowing was permitted in order that accumulated secretions from other glands of the mouth might be disposed of. The rate of secretion was recorded at the end of every minute.

When the experiment began, the normal secretion for fifteen minutes was recorded and served as the control rate for the subsequent tests. The subject was then instructed to drink two cups of coffee prepared under standard directions. One minute was allowed for the consumption of this drink. The subject then resumed his control posture while variations in the rate of secretion for the subsequent period were recorded.

As a check against any claim that our results might be due to factors other than caffeine, both warm water and de-caffeinated coffee were occasionally substituted for the caffeinated beverage. In the de-caffeinated beverage 97 percent of the caffeine is claimed to have been removed.

A typical result of the preliminary tests is shown graphically in Fig. 1. The lower graph represents the variation in secretion when a subject was given two cups of warm water. The upper graph represents the variation in secretion from the same gland of the same individual when a similar volume of coffee of the same temperature was substituted for water. For convenience, five minute periods are used in the graph rather than one minute periods. In both graphs, the first three columns show the control or normal flow for the fifteen minutes preceding the ingestion of the fluid. The 4th and 5th broken columns represent the five minutes during which the coffee or water was consumed, and the five minutes immediately thereafter during which a marked increase resulting from direct stimulation is apparent. This increase is due, of course, to taste and swallowing stimulation and since it was not under consideration in this study, these two columns were shortened

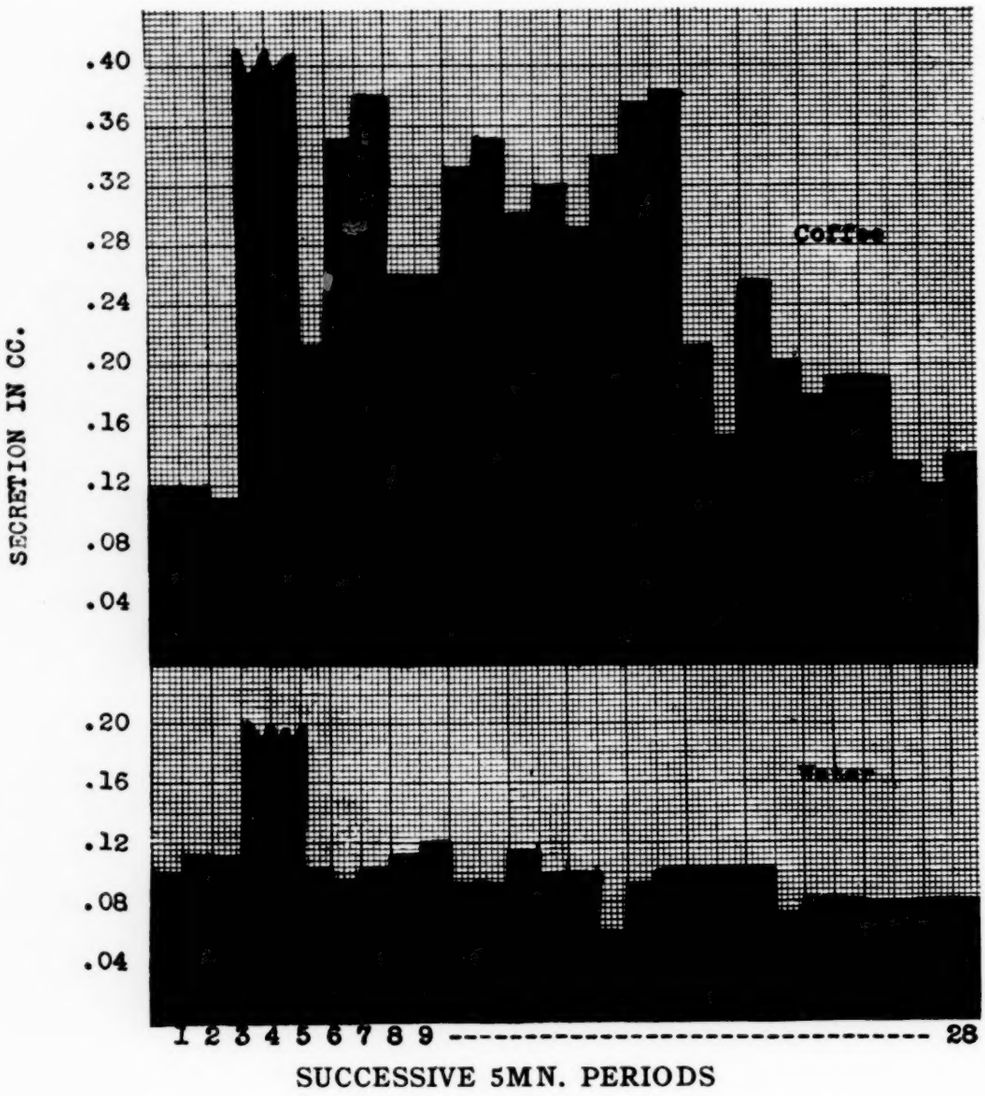

FIG. 1. The effect of coffee on the flow of parotid secretion.

in the graph to conserve space. Columns 6–28 represent the rate of secretion after the water or coffee had been consumed and the subject had assumed a control posture.

It will be observed that the ingestion of warm water is not followed by an increase in parotid secretion, whereas the ingestion of coffee is followed within a few minutes by an increase of from 100 to 200 percent which persists from a few minutes to two hours or more depending upon many different factors. Both the rate of increase and the duration of the stimulation show marked individual differences. Aside from the wide quantitative variation in glandular flow among different indi-

viduals, our experiments have shown marked differences both in rate and duration due to habituation, or the development of tolerance.

Other preliminary tests were made wherein the effect of coffee was compared with a like dosage of de-caffeinated coffee in order to determine whether or not the increase secretion following a drink of coffee was largely due to the caffeine it contained. These tests showed practically no subsequent increase when de-caffeinated beverages were used. The results when graphed were much the same as those obtained when water alone was used. Any slight increase observed could be accounted for by the fact that there was a slight amount of caffeine left in spite of efforts made to remove it. It seems highly probable that the increased secretion following a drink of coffee may be ascribed to the caffeine contained in the coffee. Other beverages known to contain caffeine showed the same results when tested.

Tests made when both habituated and non-habituated coffee drinkers were used as subjects yielded significant differences between the two groups, both as to the volume of increase and the duration of this increase. Habituated subjects showed but a slight increase in the rate of secretion and this increase was of short duration. The non-habituated showed just the opposite results. Our next problem was to study the development of this tolerance which characterized the behavior of the habituated coffee drinkers.

Accordingly a new group of subjects was selected who were unhabituated to caffeinated beverages of any kind and arrangements were made for them to consume a given amount of a commercialized beverage containing a high percentage of caffeine. This commercialized beverage was selected in place of coffee because the proportion of caffeine was well standardized and the dosage could be easily controlled. The fact that it contained a high percentage of caffeine was favorable to the experiment. Each subject was given two pints of this beverage at a given hour each afternoon. The secretion both before and after the ingestion of the beverage was carefully checked and recorded as indicated in the pre-

vious experiments. The experiments extended over a period of six weeks. Some of the results obtained from one subject are shown in Fig. 2. These results showing the salivary reaction at the beginning, in the middle, and at the end of the experiment are typical.

In graph No. 1, columns No. 1, 2, and 3 represent again the control or normal rate of secretion. Columns 4 and 5 represent the period of ingestion but column 4 represents one minute only, the time allowed for drinking the beverage. The increase shown in the subsequent five minute period, No. 5, is due to the effect of the swallowing. The low rate of secretion shown in columns 6 and 7 represents the latent period or the time elapsing between the ingestion of the beverage and the appearance of the increased secretion due to indirect stimulation. The subsequent increase in secretion continuing for approximately one hour in graph No. 1, represents the effect of caffeine on the gland.

Graph No. 2 represents the secretion flowing from the same gland when the same stimulation was used 21 days later, after the subject had been drinking two pints of a caffeinated beverage each day. Graph No. 3 represents the secretion from the same gland 19 days later (or 40 days after the experiment began) when the experiment with this subject was concluded.

A few conclusions about the development of tolerance for caffeine may be drawn from these experiments. There is a gradual reduction in the flow of parotid secretion on successive days during which, subjects were given a standard dosage of caffeine once each day. This reduction is shown in both the strength and duration of the indirect stimulation. Although the exact amount does not appear in the graph, there was also a gradual reduction of the secretion resulting from the direct stimulation as shown in columns 4 and 5. There was no variation in the length of the latent period. The elapsed time between the ingestion of the beverage and the onset of the indirect stimulation was uncanny in its regularity throughout the experiment. At exactly 15 minutes after the ingestion of the beverage the increase flow would appear. This was true both at the beginning of the experiment and after six weeks of use.

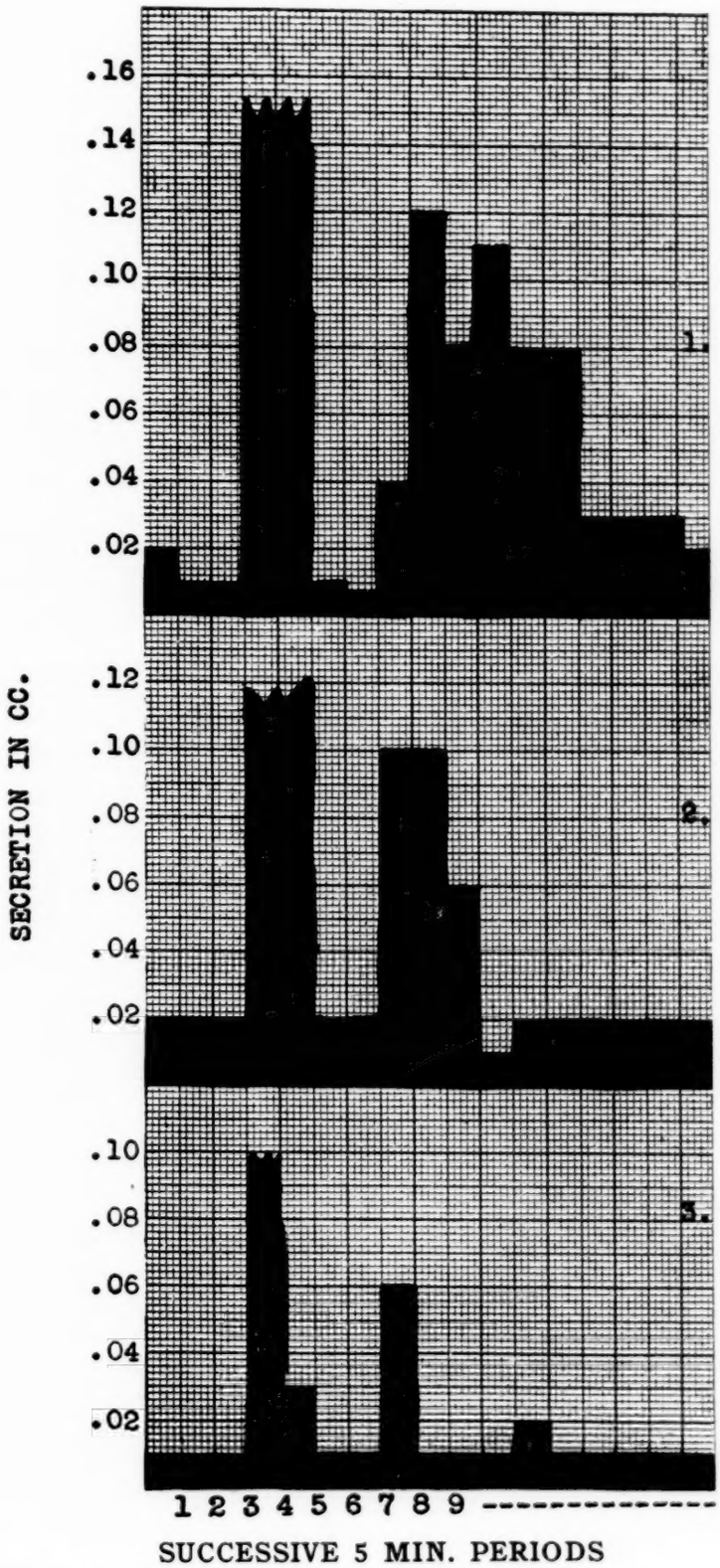

FIG. 2. The development of tolerance to a caffeinated beverage. (1) The salivary flow at the beginning of the experiment. (2) The flow for the same subject after he had consumed ½ pint of the beverage each day for 21 days. (3) The flow after 40 days during which ½ pint was consumed each day.

Although the time would vary with different individuals, the subject in this experiment developed a fairly effective tolerance for caffeine during the six weeks in which he drank one pint of the caffeinated beverage each day. Undoubtedly some individuals would develop a tolerance in much less time than this, whereas others may require a longer time. More exact data on the range of individual differences must await further experimentation.

Apparently the organism develops a tolerance for a specific dosage of caffeine. If the amount to which one is habituated is increased the digestive juices are disturbed again much the same as for the uninitiated. At the conclusion of the experiment with a non-user of caffeine reported above, a subject was selected who for several years had drank normally two cups of coffee per day. When this amount was doubled and administered daily under laboratory conditions, the parotid secretion increased very much the same as shown in graph 1, Fig. 2. This double dosage was continued daily for three weeks at the end of which time the amount and duration of the flow of secretion resembled that shown in graph 3, Fig. 2. This finding supports data reported by Hawks (**3**) wherein coffee drinkers who had taken normal amounts of coffee for years showed the same physiological and psychological disturbance as the novice, when their daily consumption was increased one to three cups.

The generally recognized influence of caffeine on sleepiness was clearly demonstrated at various stages of the experiments. Due to the facts that the tests were made late in the afternoon and that the subjects were required to sit quietly for a long period of time, they would occasionally become drowsy and begin to nod their heads. At the onset of the increased secretion following the latent period there was every evidence that the sleepiness disappeared and the subjects later reported that they were no longer drowsy. Obviously whatever immediate influence caffeine may have on the organism, whether physiological or psychological, begins with the absorption of the caffeine into the blood.

From these data it would seem evident that at least some

of the disagreement among investigators of the influence of drugs on behavior would be eliminated if the capacity of the organism to develop tolerance were better understood. By no means all of these differences in the reactions to caffeine can be accounted for in terms of tolerance, however. Just as we have found marked individual differences in motor reactions, we found marked individual differences in secretory activity. The normal rate of secretion for some individuals was observed to be ten times as high as that for others. All of us have observed gross individual differences in the functioning of sweat glands. Apparently the differences in the functioning of parotid glands are equally pronounced. Should these same differences be found in the case of the endocrine glands, it would provide a further step in explaining the role they play in the metabolism of the organism.

In so far as quantity of secretion plays a part in the digestive processes, some inferences may be drawn from these experiments relative to the effect of caffeinated beverages on digestion. By stimulating more secretion these drinks would aid digestion, provided the advantages of the increased flow were not counteracted by chemical changes incident to the ingestion of the beverage. The significance of these beverages as an aid to digestion would decrease, as tolerance developed through habituation.

These data lend support to those investigators who have found caffeine to be a stimulant. In general physiological experiments have shown that the administration of caffeine quickens the functioning of the nervous processes, thus shortening reflex excitability and increasing heart action. To this list of processes, known to be accelerated by caffeine, we may now add digestive juices. Although our experiments dealt with salivary activity only, this indirect flow of these glands cannot be regarded as anything else than a component of the alimentary reflex. The evidence seems fairly clear that at least the juices of the stomach are also increased.

At the present time similar studies are being made in our laboratory with tea, tobacco, and alcohol. In addition to determining the time necessary to develop tolerance for these

drugs, an attempt is being made to isolate the many factors which affect this development and to determine the extent to which a developed tolerance for one drug will influence the reaction to other drugs. Changes in motor activity as well as secretory activity are being studied.

(Manuscript received May 17, 1932)

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CHANGES IN GRIP TENSION FOLLOWING ELECTRIC SHOCK IN MIRROR TRACING [1]

BY WILSON McTEER

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Two problems were involved in this experiment: first, the effect of electric shock punishment for errors upon time and accuracy in mirror tracing; second, the changes in muscular tension which occur in the inactive hand when electric shock punishment is administered in a learning problem of this type.

Apparatus

The apparatus used in this experiment may best be described from two points of reference, first, the apparatus with which the subject was directly concerned, and, second, the recording and punishing apparatus which the experimenter supervised during the experiment.

The subject was seated before a table on which was clamped a six-point star pattern.[2] This pattern consisted of a bakelite plate to which two brass plates were fastened, leaving a groove of one-half inch width and one-fourth inch depth between the plates; the total length of this groove was twenty-one inches. The subject's direct view of the star pattern was obstructed by an adjustable screen. A vertical mirror was placed a few inches beyond the pattern. On the wrist of the subject's active hand was a copper wristlet from which a flexible wire led off to a binding post on the bakelite plate. The stylus used had a metal tip of one-fourth inch diameter with a bakelite handle. A flexible wire was attached to the tip and led off through the hollow handle to a second binding post on the bakelite plate. A bakelite ferrule kept the subject's hand from coming into contact with the metal part of the stylus. The shock punishment was administered through a small metal tab on which the subject's index finger rested. The subject's inactive hand held an oval rubber bulb. A flexible rubber tube led from this bulb to the recording tambour.

Now, shifting from the subject to the experimenter as the point of reference, we find that the experimenter had to supervise three aspects of the situation: first, the control of the shock punishment; second, the recording of time and errors; and third, the recording of the changes in the grip pressure of the subject. These three phases of the apparatus will be considered in the order mentioned.

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[1] Reported on September 10, 1932 at the 40th annual meeting of the American Psychological Association held at Ithaca, New York. Reported earlier at the meeting of the Psychology Section of the Michigan Academy of Science on March 18, 1932.

[2] The experimenter is indebted to the Psychology Department of the University of Chicago for the loan of this piece of apparatus.

The current used for punishment was the ordinary 110 volt 60 cycle lighting current reduced through two rheostats (1400 ohms, 0.54 ampere and 770 ohms, 0.77 ampere connected together in parallel) which were in series with the primary of a small induction coil; the output of the secondary of the induction coil was set at 15 volts with the voltmeter connected in a shunt circuit. This voltage was chosen after a series of preliminary experiments had indicated that it was sufficiently intense to be felt by all, but was not so severe as to cause disruption of the tracing activity.

Continuous records of time, errors, and tension changes were taken with glass capillary ink pens on a continuous record kymograph. The time record was obtained by connecting a seconds pendulum with an electric marker. The beginning and end of each trial was recorded by the closing and opening of this circuit. The kymograph was not stopped between trials.

The recording of errors and the administering of punishment for errors were accomplished by the use of electric relays. When the subject touched the side of the star pattern, a circuit was made which closed two relays. The closing of the first relay completed another circuit which actuated an electric marker and recorded an error on the kymograph tape. The closing of the second relay completed the circuit from the pole of the induction coil through the relay, through the subject's index finger, through the wristlet, and back to the other pole of the induction coil. The shock was felt most strongly where the finger was in contact with the metal tab on the stylus.

The recording of grip tension occurred when the subject squeezed the bulb which was loosely held in his inactive hand. An air vent of small diameter located inside the connecting tube limited the speed of the air flow from the bulb to the tambour. It was possible for these changes in grip tension to occur both during the trial and between trials; so, as mentioned above, the kymograph was allowed to continue between trials in order to get a record of such changes as might occur during this interval.

Subjects

Twenty-three students from the author's introductory class served as subjects in this experiment. They had had no experience with learning problems of this type. Prior to the experiment the subjects had been warned that electric shocks might be used during the course of the learning. The records obtained on three subjects could not be used, two on account of apparatus difficulties, and the third as a result of emotional difficulties such that the subject did not complete the requisite number of trials. The remaining twenty subjects were equally divided between the punishment and non-punishment groups.

Procedure

The procedure followed with each subject will next be outlined briefly. The subject, on entering the experimental room, was seated before the table on which was located the star pattern, and the copper wristlet was snapped on his right wrist. After he had read a sheet of typewritten instructions, he was allowed to ask questions about any point that was not clear to him. Next, the subject's right or active hand was placed on the stylus, and the stylus was placed in the groove of the star pattern against the block that marked the beginning. Following this, the rubber bulb was placed in the left, or inactive, hand. Then, as soon as the experimenter had checked the recording apparatus to see that all of the pens were recording properly, he gave the signal to start. The time recording circuit was closed when the subject started and opened when he finished. An interval of rest was allowed between the trials. During this interval

the experimenter chatted with the subject about topics of common interest. Each subject was given fifteen trials. At the end of the fifteenth trial each punished subject was handed a rating scale [3] and asked to identify the best description of the intensity of the shock as he had experienced it, and this judgment was recorded on the kymograph tape.

Results

The results obtained will be discussed in reference to the two problems mentioned in the introduction to this paper. First, what is the effect of electric shock punishment for errors upon time and accuracy in mirror tracing? [4]

In Diagram I are given the curves for average time per trial. The solid line represents the average for the non-punished group, and the broken line shows the average for the punished group. Here it may be clearly seen that the effect of the punishment was to increase the average time per trial.

In Diagram II are given the curves for average number of errors per trial. Here there are two interesting points to be noted: first, that the punished group (the broken line) made more errors than the nonpunished group in the first two trials; second, that in the third trial and thereafter the punished group made consistently fewer errors per trial than the control group.

It is interesting to point out in passing that there was a positive rank order correlation of .75 between the intensity of the punishment, as reported by the subjects, and the number of errors in the first two trials. This relationship would suggest the hypothesis that when punishment is introduced in the initial trials of a learning problem of this type, the more severe the punishment the greater will be the disruptive effect which it will exert during this initial period.

Returning now to a more general statement we may summarize our findings as follows: The effect of electric shock punishment in this type of mirror tracing is to increase the time per trial and to reduce the number of errors per trial.

[3] McTeer, W., A Study of certain features of punishment in serial learning, *J. Exper. Psychol.*, 1931, **8**, 453–76.

[4] Critical summaries of the published studies of the effect of electric shock punishment upon human learning may be found in both:
Diserens, C. M. and Vaughn, J., The experimental psychology of motivation, *Psychol. Bull.*, 1931, **28**, 15–65, and McTeer, W., *op. cit.*

DIAGRAM I

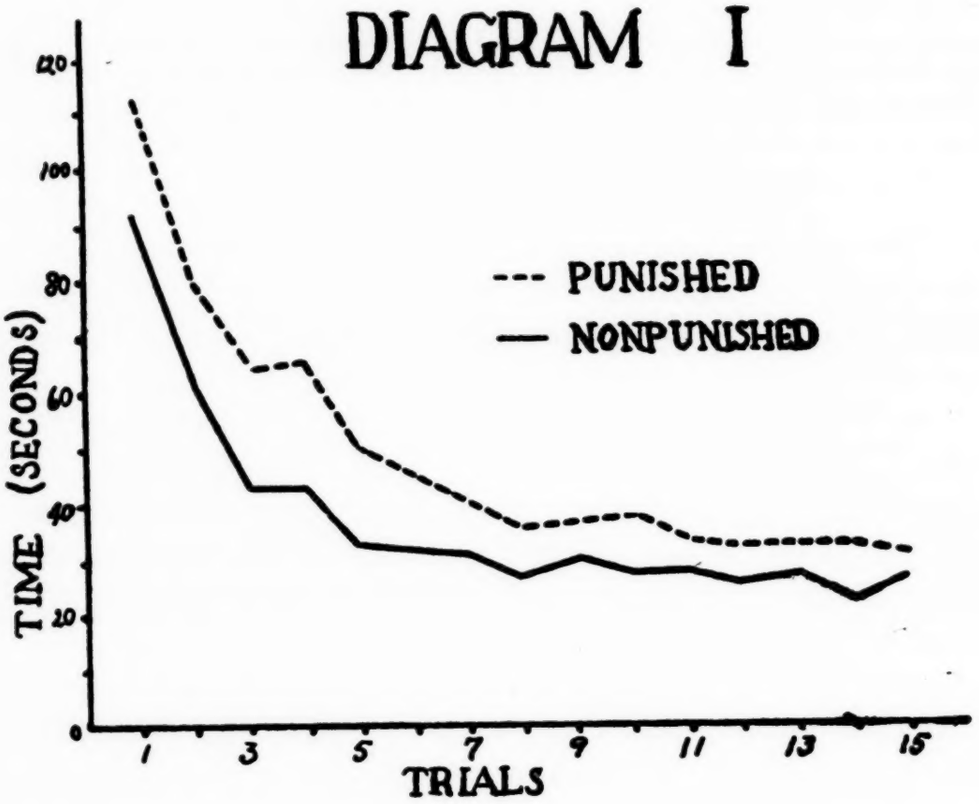
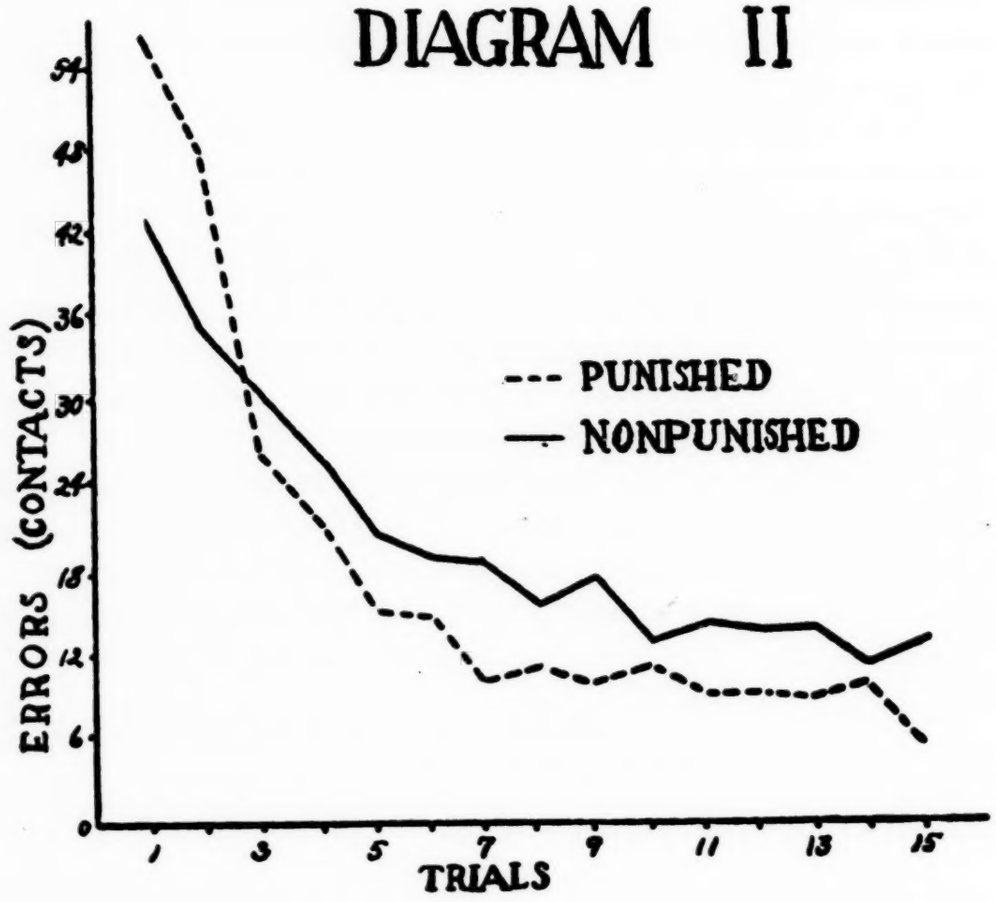


DIAGRAM II



The second problem of this investigation may be stated in the following question: What changes in muscular tension occur in the inactive hand when electric shock punishment is administered in a learning problem of this type?

A brief sketch of the background of this problem is pertinent at this point.

In 1927, A. G. Bills [5] of Chicago reported a study in which he found that artificially induced tension increased efficiency in learning nonsense syllables.[6]

During 1929–30, J. B. Stroud,[7] working under Bills, carried out a study of stylus maze learning using a special stylus which recorded changes in the tension of the active hand. Stroud found that tension was greater during the initial trials than during the later trials. He also found that greater tension was exerted when the subjects became lost or confused than when they were fully oriented.

This work of Stroud's, plus some personal observations of the tense behavior of individuals who were receiving electric shocks for errors in a learning problem, brought up the question of the relationship between the three factors: punishment, tension, and speed of learning. Accordingly, the present experiment was set up partly with the view of gathering some information relative to this question.

Following this brief introduction we may further divide our second problem into two sub-problems: First, what is the relative incidence of the deflections of the hand-bulb tambour line which occur during the actual tracing activity? Second, what is the relative incidence of the deflections of the hand-bulb tambour line which occur between the trials?

Diagram III presents histograms giving for each successive trial the number of individuals in each group who show 'divergence.' By 'divergence' is meant that deflection of the tambour line which occurred during a given trial. When such divergence occurred, it was usually of the type shown in Dia-

[5] Bills, A. G., The influence of muscular tension on the efficiency of mental work, *Am. J. Psychol.*, 1927, **38**, 227–251.

[6] See also Freeman, G. L., The spread of neuro-muscular activity during mental work, *J. Gen. Psychol.*, 1931, **5**, 479–494.

[7] Stroud, J. B., The role of muscular tensions in stylus maze learning, *J. Exper. Psychol.*, 1931, **14**, 606–631.

gram IV; that is, there was a steady and gradually increasing deflection throughout the trial. As is evident in Diagram III, divergence occurred more often with the punished group than with the non-punished group.

Now, a careful study of the apparatus used will disclose that the divergence in the first trial was caused in part by the warming of the air column enclosed in the hand-bulb as well as by changes in the grip tension. If we analyze the data with the purpose of separating these two factors, we find that the deflections which occur in the first trial without a subsequent return toward the starting level at the end of the trial may be attributed to the initial change in temperature, while those which show such a return toward the starting level may be attributed to changes in grip tension. Since we are interested in grip tension changes primarily, we are justified in reorganizing our first trial data to leave out those divergences caused by temperature alone. The results of this reorganization are shown by the double lines on Diagram III. This reorganization brings out even more clearly than before the tendency of the punished individuals to show more tension in the inactive hand than do the non-punished individuals.

As stated above, our second sub-problem is: What is the relative incidence of the deflections of the hand-bulb tambour line which occur between the trials?

In Diagram V are given histograms which show the relative incidence of 'squeezing' in the two groups. By 'squeezing' is meant that fluctuating change in pressure which occurred between trials. This fluctuating change is illustrated in Diagram VI. As is evident in Diagram V, squeezing occurred more often in the punished group than in the non-punished group. Aside from its relationship with punishment the significance of this squeezing phenomenon is not clear. There is little or no relationship between squeezing and divergence, between squeezing and time, or between squeezing and errors. Possibly this 'squeezing between trials' is an indication of 'general nervous tension'—whatever 'general nervous tension' may be.

In general, then, two types of changes in muscular tension

DIAGRAM III

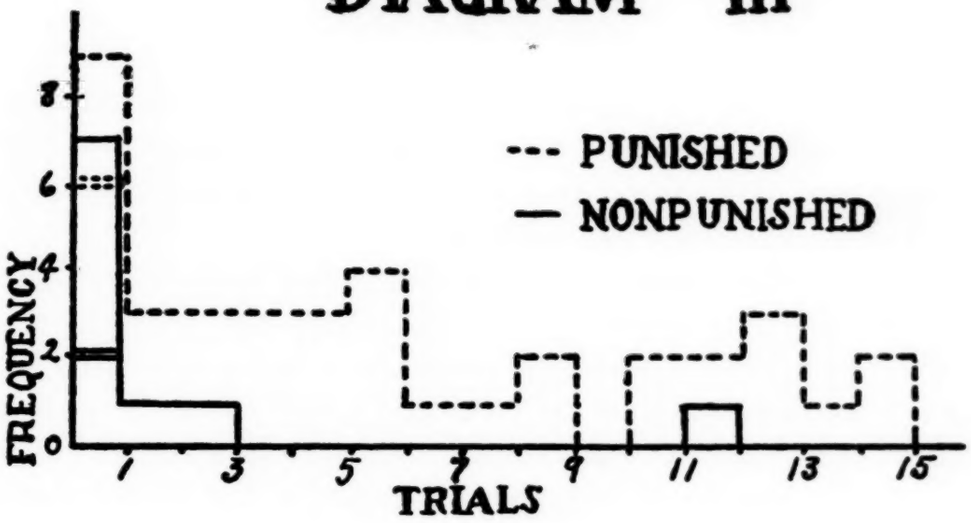
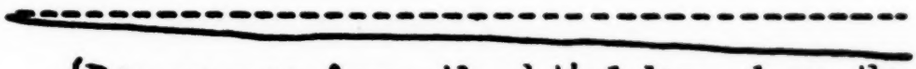


DIAGRAM IV



(Divergence from the dotted line shows the tension exerted <u>during</u> a single trial.)

DIAGRAM V

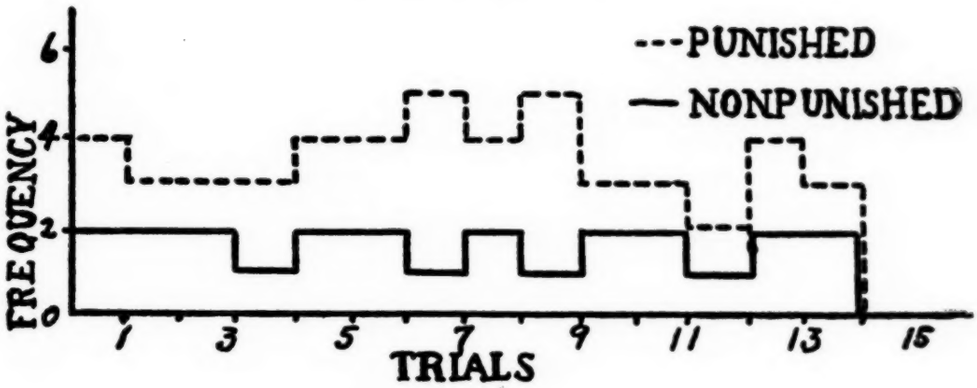
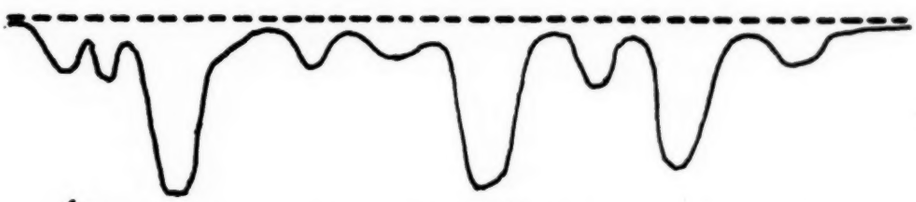


DIAGRAM VI



(Divergence from the dotted line shows the fluctuating tension exerted <u>between</u> trials.)

occur in the inactive hand during this sort of mirror tracing. The first is a gradual increase in tension during the actual tracing, and the second is a fluctuating squeeze-and-release which occurs between the trials.

The fact that tension changes of the first type occur more often with the punished group than with the nonpunished group indicates that the punishment is a factor in producing the tension, and suggests the possibility that the greater time and fewer errors of the punished group may be partly a function of the tension rather than a result of the punishment alone.

Summary

In conclusion we may summarize the results of this experiment as follows:

1. The introduction of punishment for errors in mirror tracing results in an increase of time per trial in all trials, and a decrease of errors per trial in all except the first two trials.

2. Such punishment also tends to produce greater tension in the inactive hand during the course of the tracing activity.

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THE D. L. FOR CUTANEOUS TWO-POINT STIMULATION BY THE METHOD OF SINGLE STIMULI

BY F. D. FRY, D. D. M. HAUPT AND L. WARTENA

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Fernberger [1] has described the practice of the method of single stimuli employing lifted weights as the experimental material. With this method the standard stimuli are eliminated from the experimental procedure and a varied series of stimuli are presented in irregular order to the observer who reports each as great, small or intermediate. The results are treated in accordance with the practice developed by Urban for the method of constant stimuli. It may be pointed out that the elimination of the standard stimuli reduces the time required for experimentation by exactly one half. Repetitions for the method of single stimuli for lifted weights have confirmed Fernberger's results.[2]

It has seemed worth while to apply this method to some other sensory field and, for this purpose, we have chosen the cutaneous two-point difference limen. A series of seven esthesiometers were prepared with separation differences of 1/8 inch. The smallest separation varied for the different observers but, in each case, was greater than the threshold for two-points. These were applied to the under side of the right forearm in an arranged order. The observers were required to immediately give judgments in terms of the three categories of 'long,' 'short' and 'intermediate.' The usual precautions were taken for cutaneous work. Three observers were employed and three series of judgments were

[1] S. W. Fernberger, 'On Absolute and Relative Judgments in Lifted-Weight Experiments,' *Amer. J. Psychol.*, 1931, **43**, 560–578.

[2] E. Davies and W. W. M. Babb, 'Concerning the Absolute Method in Psychophysics,' *Amer. J. Psychol.*, 1932, **44**, 367–369. S. W. Fernberger and F. W. Irwin, 'Time Relations for the Different Categories of Judgment in the "Absolute Method" in Psychophysics,' *Amer. J. Psychol.*, 1932, **44**, 505–525.

obtained from each. Each series consisted of 100 judgments on each of seven stimuli.

Observers	Series	h_1	h_2	L_1	L_2	Interval of Uncertainty	Point of Symmetry
1	1	1.26	2.29	1.21	1.47	0.258	1.378
	2	1.67	2.23	1.12	1.39	0.266	1.274
	3	2.14	3.10	1.12	1.28	0.164	1.234
2	1	2.32	2.58	1.79	2.05	0.258	1.930
	2	2.91	3.02	1.75	1.99	0.237	1.890
	3	1.25	2.94	1.62	1.99	0.364	1.880

The results of two observers gave observed relative frequencies that closely approximated the curves of the psychometric functions. The results for the third observer varied so widely from the curves of the psychometric functions that they will not be reported here.

In the accompanying table will be found the statistical constants for the results of the two observers whose results were retained. It will be observed that these results are entirely in conformity with those obtained by this same method for lifted weights with the exception of the results for the point of symmetry. For lifted weights it was found that the point of symmetry was always less than the central value of stimulus in an amount that almost exactly corresponds to the usual variation normally ascribed to the time error in the first order. In the present case for cutaneous stimulation the amount of variation between the values of the point of symmetry and the central value of stimulus is small for both observers. In the case of Observer 1, the point of symmetry is smaller by 0.08 inch than the central value of stimulus and for Observer 2, it is larger by 0.025 inch.

(Manuscript received June 10, 1933)